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The Journal of HEREDITY

A monthly publication devoted to Plant Breeding Animal Breeding and Eugenics

AN EXAMPLE OF HYBRID VIGOR

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Determining the Identity of Twins
Barbara S. Burks *Vs* H. J. Muller

Heterosis in Cow Peas
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How Races Develop

Genetics of False Wild Oats
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News and Notes

JUNE, 1926 Vol. 17, No. 6

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The

Journal of Heredity

VOL. XVII, NO. 6 JUNE, 1926

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The JOURNAL OF HEREDITY is published monthly by the
AMERICAN GENETIC ASSOCIATION

At 1918-32 Harford Avenue, Baltimore, Md., and
Victor Building, Washington, D. C.

Address communications regarding editorial matters and the business management of the Association to Room 306, Victor Building, Washington, D. C.

Entered as second class matter February 24, 1923, at the post office at Baltimore, Maryland. Acceptance for mailing at the special rate of postage provided for in Section 1103, Act of October 3, 1917, authorized December 20, 1918. **Contents copyrighted 1926 by the American Genetic Association. Reproduction of articles or parts of articles permitted only upon request, for a proper purpose, and provided due credit is given to the author and to the JOURNAL OF HEREDITY, Washington, D. C.**

Date of Issue of This Number, August 3, 1926

$3.50 *a Year; 35c the Copy*

ABNORMAL GROWTH OF THE BLACK RASPBERRY

Frontispiece

During the 1925 fruiting season the above abnormal growth was observed in one black raspberry seedling of unknown parentage, at the U. S. Horticultural Field Station, Glenn Dale, Maryland. Not all, but a large percentage of the fruits on the plant showed this development—a peduncle and fruit developing in place of a drupelet. No other abnormality appeared on the bush and whether due to contageous disease or to sporting is not known. Photo by J. Marion Shull.

George M. Darrow.

DETERMINING IDENTITY OF TWINS

A Note on "Mental Traits and Heredity" by H. J. Muller

BARBARA STODDARD BURKS*
Stanford University

THE paper by H. J. Muller in the December, 1925, issue of this *Journal* reports data on identical twins reared apart which are of extreme interest to students of the nature-nurture problem. Though based upon only one pair of twins, the data gathered are full enough to constitute real evidence. In physique and intelligence the two young women, identical twins whose schooling and experience had been notably different in many respects, were almost of a single pattern. In temperament the twins were also much alike, though differences appeared, most of which could be traced to differences in environment. Rather too much emphasis was perhaps given by the author to the differences revealed upon two so-called tests of temperament—the Downey Will—Temperament, and the Pressey test of emotional and social attitudes. The reliabilities of these tests are so low that it is probable if one twin had taken them in two forms instead of both twins taking them once, the differences between the scores would have been almost as large!

Of fully as much interest as the data gathered from the twins, is a general method proposed by the author (pp. 435 ff.) for determining the probability that a given pair of twins is of the "identical" rather than of the "fraternal" type. Such a method would be of immense value to geneticists and psychologists who need a criterion for establishing the genetic identity of pairs of twins.

The method suggested, however, suffers from certain limitations that hamper its usefulness and raise serious doubt as to whether we are justified in agreeing even approximately with the author when he says: "We should find in a collection of * * * (772) pairs of twins * * * 386 identicals, all agreeing closely, and 386 non-identicals of which only one pair agreed as closely as in the case observed. Hence, amongst each 387 that resembled each other by the required amount, only one pair would be non-identical, and the chance that the observed pair was one of the identicals would be 386÷387."

Muller arrives at this statement of probability by establishing categorical levels for six physical traits, and classifying the twins and three available sibs of the twins within the categories. On what basis the categories are set is not told, though the resulting "probabilities" depend heavily upon where the dividing lines for the categories are drawn.

His next step is to compute the chance that both twins would fall in the same category for a given trait if they were ordinary sibs instead of identical twins. He does this by first finding the proportions of the five sibs (including the twins) who fall in each categorical level. (This happens to be, in the case of the trait "body build," two sibs in the "heavier" category, and three sibs, including the twins, in the "lighter" category.) It is a simple matter then, to compute the chance that any two of the sibs would happen to fall in the same category.

This step is repeated for each trait in turn, and the product of all the re-

*Transmitted to the Editors by Lewis M. Terman.

sulting chances is taken as the probability that the twins would resemble each other by chance in all six traits.

Careful consideration convinces us, however, that the "chance" (as it has been computed) of the twins falling in the same category for a given trait is in no sense a "chance" in the usual meaning of the word. For a questionable assumption has been made: namely, that the sibs of the the twins resembled the twins *in this particular case* no more nor less than the average amount that sibs at random resemble each other, though at the same time the method correctly admits that the twins (if they were fraternal rather than identical) might resemble each other by chance either more or less than ordinary sibs do.

To illustrate the fallacy contained in this reasoning, let us take as a simple example a case in which a pair of twins have just one available sib for comparison. It is fair to do this, since Muller states that his method can be used "in all cases where a pair of twins have another sib concerning whom information can be obtained . . .", and the same arguments would apply to more complicated examples.

If this one sib were of the "heavier" build and the twins were of the "lighter" build, the chance by Muller's method that a specified child would happen to fall in the "lighter" class would be 2/3, and that he would happen to fall in the "heavier" class, 1/3. The chance of two specified children (i. e. the twins) falling in the same class would be 4/9+1/9=5/9.*

But if this one sib (as might easily happen by chance) were in the "lighter" class himself, the chance of any one of the sibship falling in the "light" class would be unity, and thus the chance of the twins both falling in the same class would be unity.

Supposing, as often happens, that the sib was by chance strikingly similar to the twins in a considerable number of the traits that were measured,—body build, color of hair, and shape of nose, it would follow by Muller's reasoning that the similarity of the twins upon those points would have no significance in establishing their genetic identity. The "chance" that they were identical twins would be spuriously low. In the case of another pair of twins, perhaps resembling each other less closely than the first pair, but having a sib who was by chance quite different from themselves, it would follow that the probability of genetic identity would be spuriously large.

A system depending upon the chance likeness or non-likeness of non-twins evidently has serious drawbacks. For obviously the "probability" of genetic identity of pairs of twins, even if all pairs showed the same amount of real similarity, would see-saw widely as the resemblance of sibs to the twins varied by chance from average sib resemblance.

However, the method could perhaps be adapted to meet the long-felt need of researchers doing work with twins. A tentative suggestion would be that the actual correlation for sibs in general be experimentally determined for the traits under considera-

*A question arises regarding Muller's procedure which is independent of the doubtfulness of the method as a whole. Starting with the knowledge that two of the three children fell in the "lighter" class, he would say that if either twin fell in the "lighter" class (probability 2/3), this being true, the chance that the other twin would be the sib also falling in the "lighter" class would be 1/2. But this makes the assumption that in a given sibship having certain potentialities, the birth of a child showing one characteristic makes it less likely that subsequent children will show the same characteristic. An analogous assumption would be that after a coin had been tossed and displayed a series of "heads" all in succession, the probability of the coin showing "tails" on the next toss would be much greater than at the beginning of the series of "heads." Fortunes have been lost at Monte Carlo through such animistic conception of probability.

tion. It could then be said for a given pair of twins falling in one of the traits at a given section, z, of a normal frequency distribution or of a "unit character" distribution; the chances are 1 in x that the sib of an individual at level z would be as close to level z as this case is. This reasoning might be carried out for as many traits as could be measured, and, allowing also for possible inter-correlations between traits, combined in a single "probability" that would have a high validity.

METHODS OF STUDYING TWINS

A Reply

H. J. Muller
*University of Texas**

A LETTER to the author from Professor Terman, accompanying a copy of the article written at the latter's suggestion by his student, Miss Barbara Burks, for this issue of the *Journal of Heredity*, has led the present writer to believe that he should explain in greater detail some points in connection with his methods of estimating in quantitative terms the probability for twin identity—inasmuch as the account in the original article was purposely much condensed, and did not discuss fully the theoretical background of the methods presented. Various methods of reckoning a minimal "probability" for genetic identity could be devised each of which would be correct in its way (according to the particular facts and relationships taken into consideration), but all methods based upon "traits" would have to employ as their data the resemblances between the twins, as compared in some way with the resemblances between *other sibs*—either those present or expected in the same family, or those in the population at large. This basic feature of the author's methods, which is apparently acknowledged as correct by Burks also, seems so obvious (when once pointed out) that it is strange an attempt was not made long ago to work out a quantitative measure from it, especially considering the various specific methods of measurement possible (the author presented two). In view of the present state of anthropometric data, however, the specific method employing artificial "categories," and based on the resemblance amongst the twins and their own sibs, will probably remain the safest for a considerable time.

This specific method is mathematically sound, in giving us very nearly minimum estimates of the probability for identity, if once the three premises of the method stated in the original article are granted to be true, and if a given ratio of identicals to non-identicals in the general population is accepted. The three premises are (1.) that non-identical twins are as likely to fall into separate "categories" (see below) as ordinary sibs are; (2.) that category limits can be "artificially" chosen for given traits in a given case, such that identical twins would hardly ever fall into different categories; (3.) that the different traits studied are inherited independently of one another. It was pointed out that these three premises (which were given in somewhat different form and designated as 3, 2, and 1, respectively, in the first article) can none of them be absolutely correct, but it was claimed that traits and category limits could be so chosen

*Department of Zoology, Contribution No. 197.

that the resulting inaccuracy would be slight; in addition, statistical methods were indicated by means of which the amount of error possible in this way could be determined, and the methods revised accordingly, so that a final, still more accurate "minimal probability" could be obtained.

These checking methods might have been treated in much greater detail and still other statistical methods of attacking the same problems might have been presented, but, in view of the fact that adequate data making possible such refinements are not on record, a lengthy discussion of the matter seemed uncalled for in the present connection. In the case of the twins and their sibs considered in the original article, as a matter of fact, the great range of sib variation permitted such wide category ranges that premises (1) and (2) almost certainly held, for the traits considered, and it is likely that in very many other families containing twins it will be similarly possible to choose categories of unquestionable genetic significance and still find that the sibs differ in a sufficient number of traits (in respect to these categories) to yield a high probability for identity.* In all such cases, then, practically the only uncertainty concerns premise (3),—on the basis of which the chances obtained for each trait are multiplied together, without taking into account any intra-familial correlation between the traits.

The presence of twenty-four pairs of chromosomes in man, and the undoubted existence, for many traits, of numerous commonly occurring gene-differences which affect one given trait without causing a concomitant change in a given direction in another given trait makes considerable independent inheritance of traits unavoidable. However, there is sure to be some linkage, and there are also some conditions like cretinism, apparently inherited rather simply, in which many traits are affected simultaneously. There are also occasional environic conditions that tend to induce non-genetic variations, such as perhaps Mongolian idiocy, adenoids, and rickets, in which

*As implied above, in the discussion of "checking methods," sufficient statistics on twins and sibs will permit us to ascertain the quantitatively narrowest legitimate limits for these categories and thus enable us to get positive evidence either for or against identity in many cases that would previously have had to be set down as "doubtful." One program by which such information could be obtained is as follows: (a) Procure abundant data giving, for various traits, the amounts of difference between twins of *opposite* sex and between these twins and their other sibs of opposite sex, and construct frequency curves of these differences, for the twins and for the sibs respectively. There is little doubt that for most physical traits of adults in which the influence of age is negligible or can be allowed for the two curves will be very similar, but the extent to which they do not agree can be determined. (b) After this has been done, a theoretical frequency curve for the differences between *non-identical* twins of the *same* sex can be constructed for these chosen traits, using as a basis curves for ordinary sibs of the same sex, and modifying these in accordance with the results of (a). (c) The actual data for twins of the same sex, identicals and non-identicals combined, obtained in such a way as not to favor the selection of identicals for the data—this can be done by following up birth records rather than by advertisement or questionnaire—may then be made into an empirical frequency curve of the differences for each trait. This curve will be a composite of nearly equal parts of a curve for identical and one for non-identical twins. (d) If then we subtract from the empirical curve—ordinate from corresponding ordinate—the "theoretical" curve for non-identicals of the same sex, taking the area of this "theoretical" curve as nearly one half that of the empirical curve (or whatever is the most probable proportion there of identicals among twins of the same sex, calculated from the sex ratio data) the remainder will be the frequency curve for differences between identical twins. This curve will show how great the differences between our "categories" should be, in order that identical twins will practically always fall in the same category. Thus we may make premise (2) strictly correct, or else determine and allow for the exact amount of its incorrectness. The data in (a) will also tell us the accuracy of premise (1), and yield a formula allowing for any inaccuracy.

certain apparently unrelated characters that ordinarily seem little correlated vary together in a pronounced fashion. Before our formula connecting the probabilities for the various traits could achieve maximum exactitude, therefore, we should have to determine the frequency of such correlative variation within families by appropriate statistical data,* but meanwhile such connected variation, in traits like those chosen by the writer, seems highly exceptional. This conclusion is supported by the analogy of genetic results obtained in other animals, particularly in other mammals, for whereas both linkage and multiple action ("pleiotropy") of genes do occur commonly when all the characteristics of the organism are considered, the frequency of occurrence of conspicuous connections between any two *given* traits is very low where the chromosome number is high.

The above three premises, the character of which as assumptions was distinctly pointed out in the original article, would indeed have afforded topics for further discussion, but these were not the objectives of Burks' (and, by implication, Terman's) criticisms—premise (1.) not at all, (2.) only indirectly and to a minor extent, while (3.) is only briefly referred to at the close of her article, in a manner which suggests that she considers it relatively unimportant. The criticisms, which in the main take the form of an attack upon the deductions and mathematical procedure following these premises, involve fallacies of mathematical reasoning that would be obvious to most persons having a background both in genetics and in the elements of the mathematics of probability, yet are too technical to be well suited for presentation to others than these. It will be necessary, therefore, for the present writer to resort to a compromise mode of treatment for which he must ask the pardon of both sets of readers.

Burks seems, firstly, to assume that the author wishes to predict the probabilities of a given sib having a specified character developed to a particular degree; this could not be done without a far greater knowledge of the minutiae of gene differences and gene action, of pedigrees and genetic formulae, in human material, than will probably be available for a century at least. Such predictions can at present be made only in very special genetic material—as in pedigreed Drosophila, snap-dragons, etc. In all such cases it is of course true (not on *a priori* grounds, but because of the mode of operation of the laws of heredity) that the chances for each sib are practically independent of, and the same as, the corresponding chances for every other sib, and this must also hold for man, even tho we cannot yet calculate what these chances are, in any but the most limited instances. Fortunately, however, light can be thrown on the question really at issue here—the chance of non-identity versus identity in twins—without the necessity of calculating the ideal frequency curve of values for each trait, that would be obtained in a given family if it comprised an indefinitely large number of individuals. For, where non-genetic influences can be neglected, the categories or measured differences being

*This cannot be found by means of the ordinary correlation coefficient, obtained from lumped data on the general population, since the gene, or the combination of linked genes, or the environic factor, having manifold expression in one family, may result in a quite different somatic complex from another gene, linkage combination, or environment affecting some of the same traits in another family. Thus, if in the first case the commoner alternatives were *AB* and *ab*, and in the second case they were *Ab* and *aB*, the mingling of families of the two types would in the general population mask any intra-family correlation. The only families that would be of use for the data would be those containing at least three sibs, two of which differed in two of the traits studied.

sufficiently wide, (premises 1 and 2), and where traits are inherited independently of one another (premise 3), the frequency distributions of character-magnitudes, or of categories, for each trait,—let us call these the "category frequencies,"—do not determine in any way their *method of assortment* or, as we may say, the "*allocations*" of these differing categories to one or another chronologically specified sib, or to one or another sib which falls into specified categories with regard to other traits. In short, *the "category-allocations" among the sibs will be independent of the "category-frequencies"; the former are random, the latter are not.* It is the allocations which are of paramount importance for our twin criterion, and these can hence be used even tho the ideal category frequencies are unknown, for the same *calculable randomness* of category-allocations should obtain in the actual samples of material which given families present, as in the "ideal" indefinitely large families. Thus, we may take the category-frequencies as we actually find them, and then reckon the chances, for families having just these frequencies, of their having one or another category-allocation. This reckoning is accomplished by the methods presented in the author's paper.

The foregoing reasoning, and that presented in the first article, nowhere involve the assumption that Burks has attributed to the writer: namely, that the sibs of a pair of twins must resemble them to just the extent that sibs on the average resemble each other. It does, however, in calculating the chances of different arrangements (allocations) of traits, make use of the randomness of these arrangements, which involves the principle that in cases of non-identicals the chance that twin A was more like twin B than it was like sib C was the same as the chance for the alternative (that A was more like C then like B), *regardless of C's character,* since an allocation of traits to B and C the reverse of what actually existed (amounting to an exchange of traits between them) would have been as likely as that which actually occurred.

When we proceed on the basis above outlined, keeping the category-frequencies—or the numbers of individuals having given character-magnitudes—constant, and equal to the frequencies observed in the given case, it is evident that the number of different arrangements or "allocations" of the traits amongst individual sibs, and with regard to the other traits, will be limited and predictable. Under this legitimate limitation in regard to frequency, if one sib falls into a certain category, the chance for another sib's doing so are not the same as if the "first" sib had fallen into a different category (despite my critic's invocation of Monte Carlo). A true analogy here would be to the process of withdrawing balls from a bag containing a limited number of balls; the chance of obtaining a red ball, on second draw, from a bag originally containing two red and three green, certainly depends materially upon whether red or green were permanently withdrawn on first draw. The chances of two chronologically specified balls—say the first and second—belonging to the same category, will then be reckoned by the same formula as that given for the twins: $\frac{a(a-1)}{n(n-1)} + \frac{b(b-1)}{n(n-1)} + \frac{c(c-1)}{n(n-1)} + \ldots$, where a,b,c, etc., are the number of balls of each category (red, green, blue, etc.) in the bag to start with, and n is the total number of balls (the sum of a,b,c, etc.). In the notation of probability mathematics, this formula represents the sum of the number of combinations of a,b,c, etc., things, taken two at a time, divided by the number of combinations of n things taken two at a time. Thus it gives the ratio of the number of possible combinations in which our two chosen individuals (or balls) can be taken from the same

category, to the total number of possible combinations of two individuals (or balls) drawn from the entire lot of n individuals (or balls). Since, further, the various traits studied are taken as inherited independently, the chances obtained for each trait may be multiplied together to obtain the chance (call it p) of two sibs (e. g., non-identical twins) agreeing in *all* the traits studied, in any family having the given category-frequencies.

Now, on the premises here assumed, the identical twins of a pair would always fall into the same categories; their corresponding chance would hence be 1. If, then, there were an equal number of identical and of like-sexed fraternal twins among families possessing the given category-frequencies, the number of cases, among such families, of non-identicals which possessed the requisite *allocation* of traits for identicals (i. e., twins agreeing in all traits) would be to the cases of identicals among such families as p: 1, and the chance, among such families, for any given case of twins, so agreeing, to be identicals would be $\frac{1}{p+1}$. This was the reckoning followed in the author's paper, inasmuch as there is evidence from sex-ratios to show that there are commonly about as many cases of identical twins as of non-identical twins having the same sex.*

This method of figuring ignores one fact: that, in cases of identicals, the category-frequencies as well as the allocations are affected by the twinning, since two individuals here vary according to the probability rules for one. This results in a greater range of variation of the category-frequencies, from family to family, in the families that include identicals; the families having the more usual values of category frequency will hence include very slightly less than their proportionate share (one-half) of identicals as compared with non-identicals, and the rarer families, possessing extreme frequencies, will contain many more cases of identicals than of non-identicals. Sometimes, then, the probability for identity, in a case where either condition is possible, will be very slightly less than $\frac{1}{p+1}$, but in other cases it will be considerably greater; the error so caused, where important enough to be considered, is hence on the side of safety.

That the author's critic (or critics) has failed to grasp the principles of his method is shown by the fact that the hypothetical illustration presented by her is incorrectly worked out. Supposing two lighter twins and a heavier sib, the chances of any two sibs chosen at random from among them being in the same category is (by formula $\left(\frac{a(a-1)}{n(n-1)} + \frac{b(b-1)}{n(n-1)}\right)\frac{2\cdot1}{3\cdot2} + \frac{1\cdot0}{3\cdot2}$ $= \frac{1}{3}$; in other words, as is fairly evident, there is one chance in three that the two "draws" shall be alike. Burks, however, takes the observed $\frac{2}{3}$ as equivalent to a theoretical expectation for "heavy," and $\frac{1}{3}$ for "light," and then

*In some special groups there are known to be more identicals than this. In any population in which sex ratio studies on twins are available the ratio of identicals to non-identicals of like-sex follows the formula $\frac{s-o}{o}$, where s is the number of cases of twins of the same sex, and o the number of cases of opposite sex. The number so obtained may then be substituted for 1 in the formula $\frac{1}{p+1}$, when the twins investigated are drawn "at random" from that population.

reckons (allegedly by the author's method), that the chance of the twins being alike is $\frac{2}{3}\times\frac{2}{3} + \frac{1}{3}\times\frac{1}{3} = \frac{5}{9}$; the chances for the different sibs are thus taken as independent here even though in a footnote she criticises the method for involving dependent chances. Her procedure is unsound because it allows the "category-frequencies" to vary, and we do not have the knowledge of genetic formulae allowing us to reckon what variation would actually take place among a group of families the parents of which were genetically constituted like the parents of a group of families showing the observed frequencies; we must confine our variation, therefore, to the allocations of the traits. Burks is not in error, however, in assuming that she may use for her illustrations cases in which there is only one sib in addition to the twins; to be sure, the probability adduced in such cases tends to be smaller, as it should be, but the product-chance will very often be significant nevertheless.

Proceeding with another illustration, Burks next assumes that all the individuals in the hypothetical family of twins and one sib are "light," and obtains the correct result (by a method not stated) that the chance of both twins having been allocated the light character would have been unity. In the latter case, then, there is (in our ignorance of the parental formulae, etc.) no determinable significance in this particular resemblance between the twins. This result constitutes no argument against the method, though apparently so regarded by Burks; it was to have been expected, because if one ordinary sib actually resembles another so closely in the given family we cannot take such resemblance in that family in evidence of identical twinning. For the likeness of these sibs was not mere "chance," in the sense that an individual from any other family would be equally likely to show such resemblance; it was partly chance but it was also an indication (the only indication readily available) of lack of heterozygosity in the parents, and as such it testified to the likelihood of the twins being alike in the respect in question, no matter whether they had been identical or not. A conspicuous difference between the sibs, would, *mutatis mutandis*, render likeness of the twins more indicative of identical twinning.

Burks then supposes that "by chance" the one non-twin sib strikingly resembled the twins in a considerable number of traits, and remarks that in that case the probability calculated for identity would be "spuriously low." This involves either a misunderstanding of heredity or the old fallacy of imagining an admittedly very improbable situation and then accepting it as likely. If, among all hypothetical families having parents genetically identical with those in the family under consideration, the frequency of a "striking similarity" in some given trait (e. g., hair color) was only as low as one in four, and if the frequencies of such similarities were the same for the other traits, then, since we are admittedly assuming the traits to be inherited independently, the frequency (chance) of sibs being "strikingly alike" in all of six traits (we cannot use a very large number without running too much risk of linkage) would be only $(\frac{1}{4})^6$, or one in 4096. Such a situation is scarcely one which "often happens." A little calculation will further show that only once in 38 cases (where the expectation for likeness is $\frac{1}{4}$, for each trait) will the single sib be so much like the identical twins that the probability for identity, reckoned, by the author's method, from this one sib and the twins, will be lower than 27 to 1.

How then can we account for the cases of close sib resemblance that "often happen?" Barring non-independent inheritance, previously discussed, they are obviously referable to

a comparatively homozygous condition in one or both of the parents, i. e., to a condition in which the chance of striking resemblance would average much greater than ¼ for the given traits: a chance of about ¾ would be necessary, for each trait,—if only six traits were involved,—before such general resemblance became frequent. But in this latter case, where *any* sibs are apt to resemble each other, it is evident that a resemblance between twins *should not* be regarded as evidence of their being identical; hence the probability for identity computed on the basis of sib resemblance, while low here, is not "spuriously low."

The same line of reasoning will show that where one sib differs conspicuously from another in a considerable number of traits, the chance favoring the identity of twins that show much less difference in all these traits will not be "spuriously large," but legitimately so. Thus, as the resemblance between sibs "see-saws widely" from family to family, the significance of twin resemblance varies with it, since the see-sawing by no means represents the mere results of "chance," in the sense of simple sampling, but provides us with valuable information concerning expectations. The final answer, however, to all these criticisms, really lies in the fact previously pointed out that, whatever the expectations in particular families may be, the method does give a correct figure (granting our three premises) for the probabilities in the case of any family selected at random from a group of families having the *observed* trait frequencies. All the considerations of the four preceding paragraphs are in a sense integrated in the formula.

Burks proposes a different specific method for comparing twin and sib resemblances than that advanced by the present writer, in order to meet the alleged objections discussed above. Careful study of the implications of her method shows that it too involves the three premises of the author's method,—except in so far as statistics will render both methods independent of them. Her method, however, could not be used at all before considerable statistics were available. But it suffers from a far graver difficulty than this. For her computation goes no further than to find the frequency of each possible amount of sib-difference in the "general" population; if the twins show a given amount of difference in a trait, the table then gives the chance that non-identicals would show only this much difference. But this neglects the very pertinent fact that the genetic variabilities, and hence the expected amount of sib-resemblance, varies tremendously from family to family in a given locality, from locality to locality, from race to race, etc., according to the amount of previous mixture, the amount of recent inbreeding, the prevalence of certain trait-differences, etc. All this would result in a far greater amount of uncertainty than that caused by the allowed-for errors of sib sampling of the author's method, where all the evidence concerning the variation in parental heterozygosity from family to family furnished by the sibs themselves is utilized. We must not lose the special significance of the particular by dissolving it in the sea of the mass; neither will the rules for one mass necessarily be valid for another. These are lessons recently borne home to the geneticist, but which statistical workers in various sciences have been slow to learn.

Even if, on using Burks' method, we could rely upon the probability yielded for a given trait, it would not be legitimate to multiply the probabilities for the different traits together. Statistical proof of premise 3 would not alter this fact; neither could an allowance be made on the basis of statistically determined "inter-correlations between traits," whether within families or in the population *en masse*. For even if there were no linkage, correlation, or other such connection between *traits*, there

would still undoubtedly be much correlation between the values of *trait-differences* amongst sibs. That is, a family which shows genetic variation in one trait is more apt to show it in another also, since certain types of matings (e. g. matings of the offspring of race crosses) tend to cause variation in many characters at the same time, whereas other types of matings (e. g., matings of inbred lines) tend to result in less than average variation in many characters at once. Thus twins of a family of the latter type are apt to be alike in many respects at once, even if they are not identical, and the chance of non-identicals in such a family being alike in several traits may be vastly less than the product of the separate chances, for the general population, of the occurrence of each specified sib-difference. On the other hand, in a cross between two mulattoes, let us say, the difference between two twins would have to be very much wider indeed before their non-identity would become likely, because here all sibs would tend to differ much in many respects, no matter whether the traits were "correlated" or not. If, then, we cannot multiply the chances obtained by this method for the separate traits, it brings us into an unfortunate predicament indeed, for the individual chances, concerned with only one trait, can yield at best a very small probability in favor of identity, one so small that by itself it would usually be "non-significant." In the author's method there is no such objection to multiplying chances, since only the differences actually existing in a family are considered.

It would be possible partially to overcome the above difficulty by getting statistics on the correlation between sib-*differences*, or by finding the frequencies of sib-differences in trait-combinations, but these figures too would vary greatly from group to group in a community, and from one community to another. However, when used with caution, they might be resorted to for evidence concerning identity in those cases in which there was no other sib, or for subsidiary evidence when there was but one other sib, which "chanced" to be very like the twins. But before the chances for the alternatives of identity versus non-identity could then be weighed against each other, in any particular case, statistics showing the frequencies of identical twin resemblances (obtainable as described in footnote 1) would have to be secured. These latter curves would not be subject to the same fluctuation from group to group as noted for the sib-resemblances, although there would be some fluctuation, especially in different races (e. g. the skin color in genetically identical men of the white race might vary more obviously, due to tanning or alcoholism, than among those of the negro race).

Touching upon a totally different topic, Burks questions the psychological significance of the differences found in the "temperament" tests. While the author holds no brief for these tests, and the case of twins under discussion in fact gives strong evidence that their *genetic* significance is low, the whole question of the degree of fixity of various temperamental traits, by genetic causes, is thrown open by the results, so long as psychological significance is in fact claimed for the tests by psychologists themselves. Further investigation, along lines which the author pointed out, is therefore necessary, since we can not rest assured that assigned temperamental characters have a genetic basis. Further tests must be administered, to twins, and the amount of psychological "reliability" of the tests already used can also be more soundly determined by such work on twins. Discrepant results obtained by giving "other forms" (as Burks suggests) would not categorically disprove the reliability of the tests already used, as it would prob-

ably be difficult to get other forms which we could be sure were equivalent to the original ones. As a matter of fact, however, the twins were given part of the Pressey XO Test in two forms, A and B, as noted in the author's first paper, and the results with these two forms confirmed each other markedly in the case of both twins, as the published data indicate. Still other forms can be tried.

But it may be better, for evaluating the psychological "reliability" and significance of a test, not to try it and an assumedly equivalent test on the same person, to see if they agree, but to try exactly the same test twice, on two persons having as nearly as possible identical psychologies. As the author emphasized before, therefore, the results of Koch's work in giving these tests to identical twins *reared together* will furnish an important check on the differences revealed in the present case. Without in any way committing Dr. Koch, who is not at the moment in Texas, the present writer would say that it appeared to him, judging by the indications in her preliminary results as announced Dec. 11, 1923, to the Texas Chapter of Sigma Xi, that such twins tend to give much more similar response to these tests than did the twins B and J, who were reared apart.

Meanwhile, we have no criteria by which to judge "temperament" qualities except these tests and subjective estimates, and we must base our judgments upon the results of both of these. The subjective estimates of the twins reared apart do indeed conform with the outcomes of the tests in most respects, though the difference did not seem everywhere as wide as there indicated, and there seemed in addition to be various important points of agreement not tested for at all.

Miss Burks' Reply

THE objections which Professor Muller advances to my plan are well taken. Nevertheless the points he has raised do not invalidate the scheme, provided traits can be found for comparison which show essentially uniform sib-correlation throughout their range for the population of which a given pair of twins is a sample. But they do make it evident that long and laborious research on sib resemblance would be necessary before the method could be applied; and that the selected traits would probably have to be confined to complex ones for which individuals are practically never homozygous. The plan originally proposed by Muller is vastly more convenient and attainable.

However, despite the arguments advanced by Professor Muller in his reply, the writer is still convinced that Muller's method yields an incorrect computation of probability,—"probability" being used as the percentage frequency with which twins who show given characteristics, and whose sibs show given characteristics, are identical rather than fraternal. While his reply emphasizes a point which the writer had not taken into consideration (i. e., that unusual similarity between twins and their sibs really favors the probability that their parents lack heterozygosity, and hence legitimately reduces the probability that the twins are identical), it seems impossible to untangle the effect of homozygosity in parents from the effect of chance. Carrying his method toward the limit: if twins chanced to resemble a single available sib in most of the traits considered, the probability in families selected as comparable that fraternal twins would resemble each other so closely would be nearly unity. Now the method, using in the way it does the fact that approximately half the random cases of twins of the same sex are "identicals," makes the error

of treating these highly selected cases as if they *were* random. Muller's procedure would imply that in a collection of comparable twin and sib groups resembling one another by the amount above, nearly half the twin pairs would be fraternal, and that the probability of a given pair of twins in the collection being identical would be little more than 1 in 2. But with hazard playing a heavy part in the composition of the comparable groups of the collection, it seems inevitable that a large majority rather than half the twin pairs should actually be identical. While, as Muller has pointed out, so extreme a situation is relatively rare, it cannot be denied that chance resemblance of even small degree has its definite effect upon the computed probability. Similarly, in the case of a pair of twins whose sib was unusually dissimilar to themselves, chance and genetics would be inextricably interlocked, and the computed "probability" would have an error whose magnitude was indeterminate.

Barbara Stoddard Burks.

Comments of Professor Muller

THE matter to which Burks refers in her second note was not raised in her own previous paper but was brought up by the present writer in his preceding article (pp. 195-96 and 199), where it was pointed out that it does not invalidate the conclusions reached by his formula. It will only be necessary here to explain in somewhat more detail the reason why—as there stated—the probability adduced by the formula is really a "minimum probability," which remains "on the side of caution."

As the purpose of the formula is to decide whether it is safe to regard as identical a pair of twins which are previously known to be similar in many physical features, it is evident that in the great majority of cases to which the formula is applied there will be more resemblance shown between the sibs of the given family, including the twins (i. e., less division of the sibs into different categories, all traits considered) than in the case of sibs of the average family. But a collection of families thus showing more than the average amount of sib, including twin, resemblance (i. e., a smaller dispersion of the curve of category-frequencies) contains more than the average proportion of identical twins (more than 50 per cent) among its cases of like-sexed twins. In such cases, then, among all families having the given category-frequencies, the "chance for identity," adduced, as in our method, solely on the basis of randomness of "category - allocations," and on the "safe" assumption of only 50 per cent monozygotic origin for the like-sexed twins, is lower than the figure which we might be able to reach if we also took into account the effect of twinning on the category-frequencies as well as on the category-allocations. (For definitions of these terms see the author's previous paper.) And the greater the general similarity between the sibs of the family (including the twins), the more would the calculated probability favoring identity fall short, for this reason, of the true frequency of twin identity in families having the same category-frequencies and category-allocations as this famiiy. Therefore, in the case of data making identity uncertain, on account of the general sib similarity, this uncertainty becomes emphasized in our calculated probability figure, but a high calculated probability (like that in the writer's first article*), which will much more often be encountered when the twins are really identical, may all the more certainly be taken as strong evidence for identity.

On the other hand, in cases of families varying (even when the twins

**Journal of Heredity* 16:432-448. December, 1925.

are included in the consideration) in the direction of a greater than average proportion of sib unlikenesses, there are, *á priori*, fewer than 50 per cent of identicals among the like-sexed twins, when we consider all families having a given series of category-frequencies, regardless of their allocations. Our probability figure, calculated on the basis of 50 per cent identicals, would hence be excessive in these cases, and the amount of its excess over the value sought for would depend on the amount by which the proportion of sib unlikenesses shown in the given family exceeded the proportion shown in the average family. This would at first sight appear as a serious objection to our method. The cases to which our formula will be applied in practice, however, can never be extreme variants in the direction of sib unlikeness, since in all actual families that include twins suspected of identity there must be a considerable number of likenesses (meaning by a likeness the inclusion of any two sibs within the same category). In fact, it can be shown that in all *small* families to which the formula will be applied (families of three sibs, including the suspected twins) the proportion of sib unlikenesses will never be appreciably greater than average, and hence the threatened objection will not apply. For, in the average family, as observation shows, the ordinary sibs fall into different categories considerably oftener than into the same, for the traits which it was proposed to consider (we need pay no attention here to the actual *amounts* of difference), and, in our hypothetical family of three, including the two twins who agree, even if the other sib differed from them in all traits considered there would still be 33 per cent of agreements in categories.

Even in the case of large families, when these contain twins suspected of being identical, there can seldom be any great deficiency of total likenesses among the sibs, below the average proportion shown in families with only non-twins or fraternal twins. It is possible, however, to overcome the objection here at issue, if we so desire, through the artificial conversion of these large families showing unusual unlikeness into families of three or four—by taking into consideration only one or two of the other sibs (taken non-selectively) besides the twins. The probability then calculated would be truly minimal, as in all small families, but even then, if the twins were really identical, this "safe" figure would be found convincingly high, inasmuch as the non-twins here differed so decisively. As a matter of fact, such conversion, with its consequent discarding of much of the evidence in our favor, is really quite unnecessary in such cases, for these are the very cases in which the allocations of those few likenesses which do occur are so special as to make the twins themselves similar in most respects at once (provided the twins really are identical). These are the very cases, then, which offer the best possible evidence for identity, the evidence derived from the peculiar category-allocations being so very strong as far to outweigh the effect of our assuming 50 per cent of identicals instead of a smaller proportion. The effect, on our final calculated figure, of variations in the category-allocations, can be shown to be of a considerably higher order of magnitude here than the effects which might be caused by our allowing or not allowing for deviations in the category-frequencies.

For the above reasons, it is not worth while, for the purpose of the formula, to try to take into consideration the effect of the category-frequencies, *per se*, on the chances. This could, however, be reckoned too, by the aid of tables giving merely the frequencies with which sibs in general (non-twins) actually fall into different "categories"—i. e., classes which we choose to regard as obviously genetically differentiated. The writer has worked out a framework of the mathematical procedure required for this, but a di-

gression on this topic is scarcely called for here.

As, when we are outdoors, it is not necessary to consult the newspaper prediction to decide whether it is raining, so, in general, it is superfluous to examine all possible evidence regarding a question, when we already have reliable evidence, of convincing forcefulness. As the writer remarked in the previous paper, various other facts and considerations, besides those employed in the formula presented, could be used, in the derivation of a probability figure (*e. g.*, the above considerations based on category-frequencies could have been employed),—but, as has been shown, it is not necessary to take into account these complications, and all the statistics involved in them, to get evidence for identity that will usually be sufficiently strong (where the twins are really identical) and yet at the same time "minimal"—i. e., on the safe side (if we grant the three premises previously discussed). In the derivation of this as a minimal probability, then, "chance and genetics" are not confused: chance is an integral part of the genetic process, and is taken into account as far as necessary for the purpose in view—that of reaching a valid conclusion.

H. J. Muller.

How Races Develop

THE CHARACTER OF RACES *by* ELLSWORTH HUNTINGTON. Price $5.00. Charles Scribner's Sons. Pp. 393, 1924.

One of the few books dealing with the forces which have made mankind and the races of men what they are. It attempts to trace the effect of natural selection from the time of the origin of man to the present. In charm of style it compares with some of the work of H. G. Wells. It is a stimulating and inspiring book and will well repay the reader.

The book is the logical companion of "Civilization and Climate" which was published by the same author in 1915 and each volume discusses what Huntington believes is a great principle generally overlooked in the study of history. The central thought of the book is that:

> A race or a nation can apparently be made or marred by natural selection. Mere numbers count for nothing; in many cases a dense population is the greatest of curses, as it has been in Ireland, China, Japan, and Germany. Quality is what counts, and quality can be obtained only by diminishing the number of people who inherit low moral and mental capacities and increasing the proportion who inherit the high qualities which lead to racial dominance. In the past, without man's conscious intervention, natural selection has been actively at work, sometimes for good and sometimes for ill. We must set ourselves diligently to limit and control the future growth of population. . . . . One aim must be to insure that all newcomers to our land are of a quality *above the average* of our own people; then they will do us good not harm. Another aim must be a low birth-rate and a low death-rate, so that our population may increase less rapidly than does our command over natural resources. Thus we shall be able not only to maintain, but raise our standard of living. But a low birth-rate is of little advantage unless it is *differential;* and the differences must be of the right sort.

The author assumes the origin of man to have occurred somewhere in what is now the plateau region extending from Mesopotamia and Persia to West China, and in a warm but not a tropical climate. He speculates upon the characteristics conceived to be of importance in the evolution of man from the higher mammals, upon his early distribution over the face of the earth, and upon the effect of the glacial periods on his character. Climates and various climatic changes are considered important in selecting certain types for survival, first in the early history of man, later in the development of the races and finally among the peoples of the present. The effects of the various environments on natural selection, the reason for the state of civilization of the various

tribes of the American Indians and the manner in which natural selection has acted and is acting in China, Europe, and especially in Iceland form the body of the book.

The later chapters are given to a discussion of the selection of modern Americans. Statistics from many sources bearing on the origin of our finest racial types are brought together. Special emphasis is laid on the loss to the race of such superior strains as the officers and the soundest men by war, and of the hundreds of thousands of women school teachers who remain single.

The author feels that America has gone far on the way toward racial impoverishment and that the lesson to be learned from history is that the future growth of population must be limited and controlled.

> The best material is being selected for destruction by war, by religious zeal, by education, by a democratic social system with free opportunities for all, by industrial development, by feminism, by celibacy, by late marriages, by birth-control, by unhealthful occupations, and in numerous other ways. Modern science has scarcely touched man's own self. Our aim for ten thousand years has been the mastery of *things* rather than the mastery of *ourselves*. It seems probable that the advanced nations will attempt to control their own physical evolution. . . . . Long before society as a whole has decided what kind of people shall increase in numbers and what kind shall diminish, there will probably be significant progress in one of those great processes of selection which have been the theme of this book. . . . . A compact group of thoughtful, conscientious, progressive, and strong-minded people are acquiring a new sense of responsibility for the production of leaders for the next generation. They are the kind of people who leave the conveniences of city apartments, and move to the suburbs for the sake of the children. They put up with discomforts and annoyances in order to have homes where children will thrive. They gladly curtail parties, travel, the theater, and the club because they feel that the home and their children are of infinitely greater value.

The author necessarily includes much controversial material to which he gives his own interpretation. As an example many tables are given showing the origin of eminent people. Such tables are of value but it should be clearly recognized that many are based only on the origin of the father and do not consider the origin of the mother, who contributes as much as the father in heredity and usually more to the training of the child. Moreover, if we accept the inheritance of ability, we must then consider that there would have been an equal number of able women as of able men in any group, but because of social conditions they did not have the opportunity of becoming prominent. This would indicate that environment does have an importance comparable to that of heredity in producing able persons, and in considering the percentage of notable individuals at any period in the history of America the effect of the environment of that period must also be taken into account.

G. M. Darrow.

A View of the Whole Field

EVOLUTION, GENETICS, AND EUGENICS, *by* HORATIO HACKETT NEWMAN. Revised ed. Pp. 639, price $3.50, postage extra. University of Chicago Press, 1925.

Professor Newman's book of selected readings, which is virtually an encyclopedia of the subject, appears in a new edition, considerably rearranged and with 115 pages of new material including a superfluous chapter on the Scopes trial. In a book of this sort there is always room for difference of taste in selections, and there are a few actual errors; but on the whole the volume, which is unique in the field of genetics, will meet most requirements. There is no other one book which covers the field so completely and satisfactorily.

P. P.

PARENTS AND RECIPROCAL HYBRIDS

Figure 1

California Black-Eye parent middle left. Blue Goose parent middle right. Hybrid of California Black-Eye pollinated by Blue Goose to extreme left. Reciprocal hybrid to extreme right. Taken eighteen days after planting.

HYBRID VIGOR IN COW PEAS

FRED. W. HOFMANN
Department of Agronomy, University of Illinois

RECENTLY, numerous crosses have been made by the writer between several different varieties of cow peas in the Agronomy Greenhouses at the University of Illinois. When the F_1's were grown and compared with their parents, a striking example of hybrid vigor or heterosis was exhibited by the hybrid of California Black Eye × Blue Goose. Since no account of the occurrence of this phenomenon in cow peas has been reported as far as the writer has been able to learn, it was thought that a description of the hybrid and parents would be of interest. This description is based almost wholly on greenhouse plants.

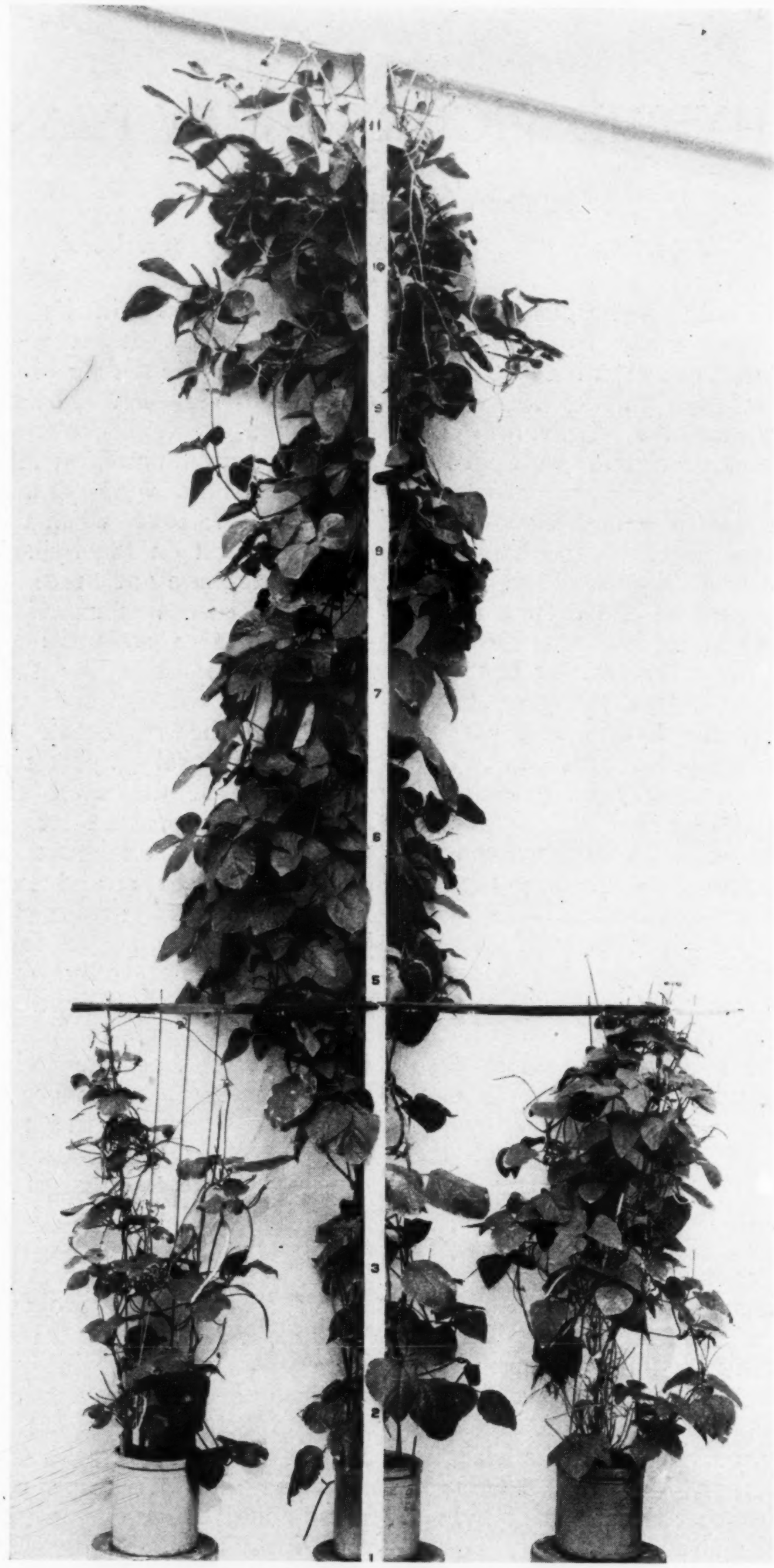
The parent varieties differ distinctly in color characters though only slightly in size. The California Black Eye variety has light blossoms faintly tinted with light bluish lavender; the blossoms appear in from six to seven weeks from planting; the seed coat of the bean is white except for a black area or "eye" around the hilum; the average diameter of the stalk at the crown is four-sixteenths of an inch, and the average height of the plant is a trifle less than four feet. The Blue Goose variety has a deep purple flower which appears in eight to ten weeks from planting; the seed coat of the bean is of a more or less uniformly speckled dark-gray-blue pattern; the average diameter of the stalk at the crown is four-sixteenths of an inch, and the average height of the plant is a trifle less than four feet. Table I gives a condensed comparison of hybrid and parental characters.

The increased vigor of the hybrid between these two varieties was noticeable rather early in its development. Figure 1 shows the reciprocal hybrids and their parents eighteen days from the date of planting. The plants shown in Figure 2 were grown from seeds that were germinated on April 7, 1925, and the plants were photographed on September 7, 1925, when the first blossom appeared on the hybrid.

To facilitate accurate comparison, particular care was exercised to maintain as nearly as possible the same growing conditions for the four different plants illustrated in Figure 1. Seeds of the respective parents and their reciprocal hybrids were all germinated at the same time. Seedlings of the parent varieties broke through the soil first and their hypocotyls were straightened out before the hybrid seedlings had fully emerged. In spite of this handicap, the hybrids not only overtook the parents, but were fully three times as tall two weeks later.

The flowers of the hybrid have a light purplish shade more or less intermediate between the two parental flower colors; the blossoms do not appear for five months, or about three months later than the first blossoms on the latest blooming parent, namely, the Blue Goose; the seed coat of the bean is black with faint brownish specks; the average diameter of the stalk at the crown is about nine-sixteenths of an inch and the average height is eighteen feet (Figure II). Since the parents as well as the hybrids behave somewhat like perennials under greenhouse conditions a comparison of yields was difficult to obtain. Some of the Blue Goose plants were a year old and still producing seed at the time when

[1]Contribution from the Division of Plant Breeding, Department of Agronomy, University of Illinois. Published with the approval of the Director of the Station.



FIVE MONTHS AFTER PLANTING

Figure 2

California Black-Eye parent to the left. Blue Goose parent to the right. Hybrid in the middle. Taken five months after planting. Note pronounced increase in height of the hybrid over that of either parent. The parent varieties blossom when about four feet high, the hybrid not until it has reached a height of eighteen feet.

the hybrids were just coming into full bloom and pod formation, and when the experiment had to be terminated for greenhouse fumigation. However, the general profusion of blossoms and pods over the hybrid plants indicated very strongly that the seed yield of the hybrid in the greenhouse would exceed that of either parent.

Under field conditions the contrast in growth between the parents and the hybrid is of a different nature. Field grown plants are less viney but stockier than greenhouse grown plants; consequently, the difference in height between the hybrid and the parents is not so marked. However, field grown hybrids have larger stems, are more branched, and appear, on the whole, to have a much stronger and more luxuriant vegetative growth than either parent (field grown). Frost killed the hybrid plants in the field before all the seed matured so that no complete record could be obtained of the seed yield. The general profusion of bloom and pods, however, indicated that the seed yield of the hybrid might exceed that of either parent. Since a heavier seed yield is indicated for the hybrid plants in the field as well as in the greenhouse, this is probably not a case of heterosis for vegetative growth only, but it also involves an increase in seed yield.

TABLE I—Giving the Outstanding Differences of Parents and their Hybrid as Grown in the Greenhouse

Characters	*California Black Eye*	*Blue Goose*	*Hybrid*
Seed Coat	White with black area or "eye" around the hilum.	Bluish gray speckled	Black with faint brownish specks
Time of flowering from planting	Six to seven weeks	Eight to ten weeks	Five months
Color of flower	Light lavender	Deep dark purplish	Light purplish
Stem size at crown	4/16 inch	6/16 inch	9/16 inch
Height of plant at blossoming	4 feet	4 feet	18 feet

The Children of Physicians

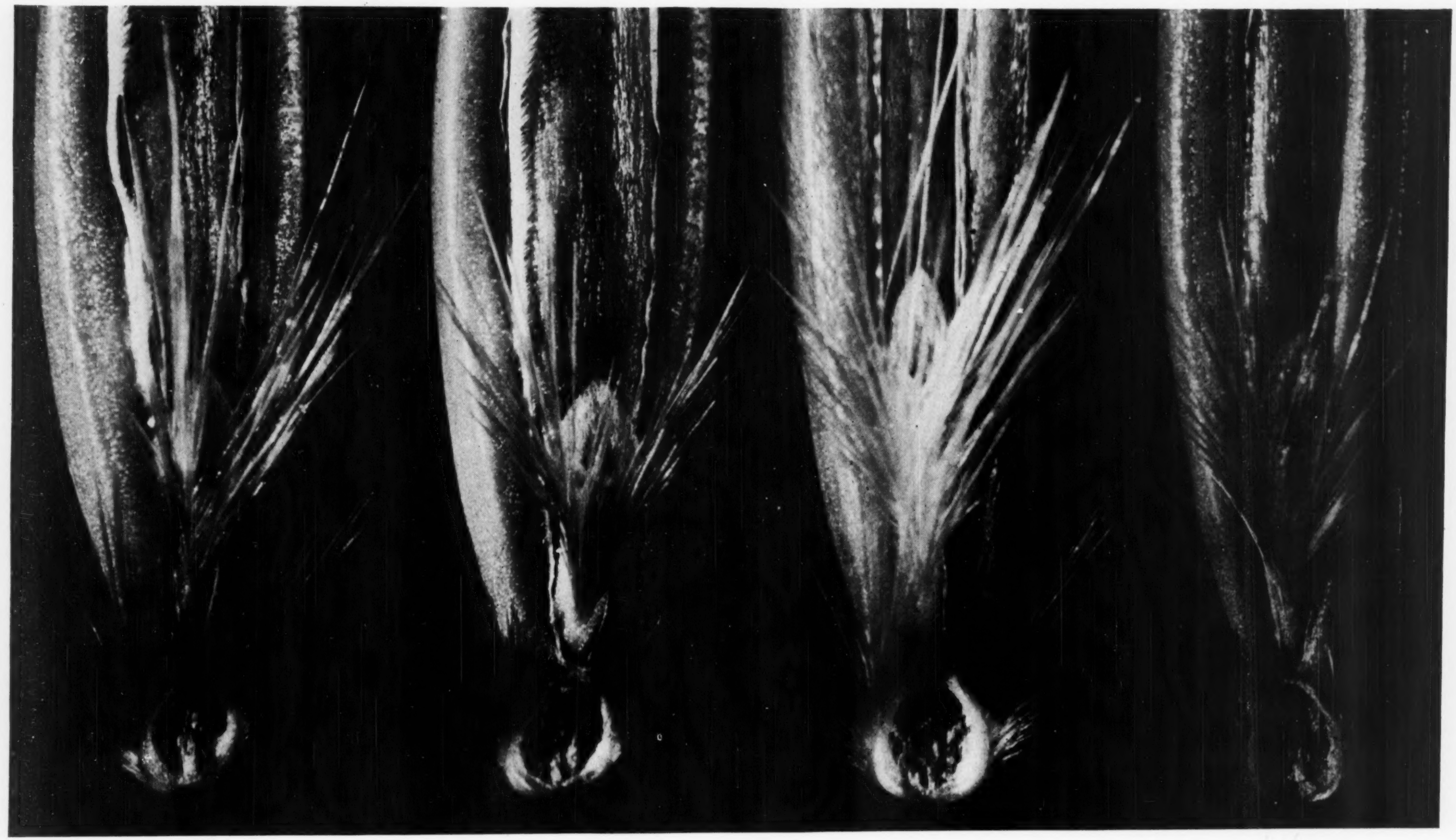
MEDICAL HEREDITY, *by* WILLIAM BROWNING. Pp. 250, price $4. Baltimore, The Norman Remington Co., 1925.

Dr. Browning has listed about 6500 children of American physicians, taking only persons who achieved some distinction. As the data are handled, it is difficult to draw any broad statistical conclusions from them, but some points of interest present themselves. Thus it appears that a taste for music and for sculpture is rather unusally common among doctors' sons. Of the individuals included in *Who's Who,* one in nineteen is the child of a physician.

P.P.

Fulghum

Burt



Long-Haired | Short-Haired | Long-Haired | Short-Haired

FATUOIDS DIFFERING IN HAIRINESS

Figure 3

Florets of the homozygous- or heterozygous-fatuoid forms occuring in the Fulghum and Burt varieties of *Avena byzantina*. In both Fulghum and Burt two types are found which differ in length of hairs on the basal callus and supported rachilla segment. The difference in hair length is very marked and undoubtedly has a genetic basis as the two forms appear to be heritable in both varieties.

FATUOID OR FALSE WILD FORMS IN FULGHUM AND OTHER OAT VARIETIES

(Continued from May Number)

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Fatuoids in Burt

A NUMBER of fatuoid or false wild forms were isolated from the supposedly pure-line selection of Burt, Kansas No. 6076,* at the Akron, (Colo.), Field Station in 1920. A few kernels of these aberrants were sown at Akron in 1921. Harvest of this material was considerably delayed, and practically all kernels of these fatuoids had fallen before a close examination of the material was made. As a result, practically no data were obtained on their breeding behavior in 1921.

None of the aberrants were sown in 1922, but in 1923 a total of 31 progeny was produced from the seed of 8 of the fatuoid plants selected in 1920. With one exception, the 8 plants bred as genotypically homozygous fatuoids. The exceptional plant produced only two progeny plants, one a fatuoid similar to the parent and the other the heterozygous-cultivated form. Although these data were limited they were sufficient to indicate that the fatuoids found in the Burt variety might be genetically similar to those occurring in Fulghum.

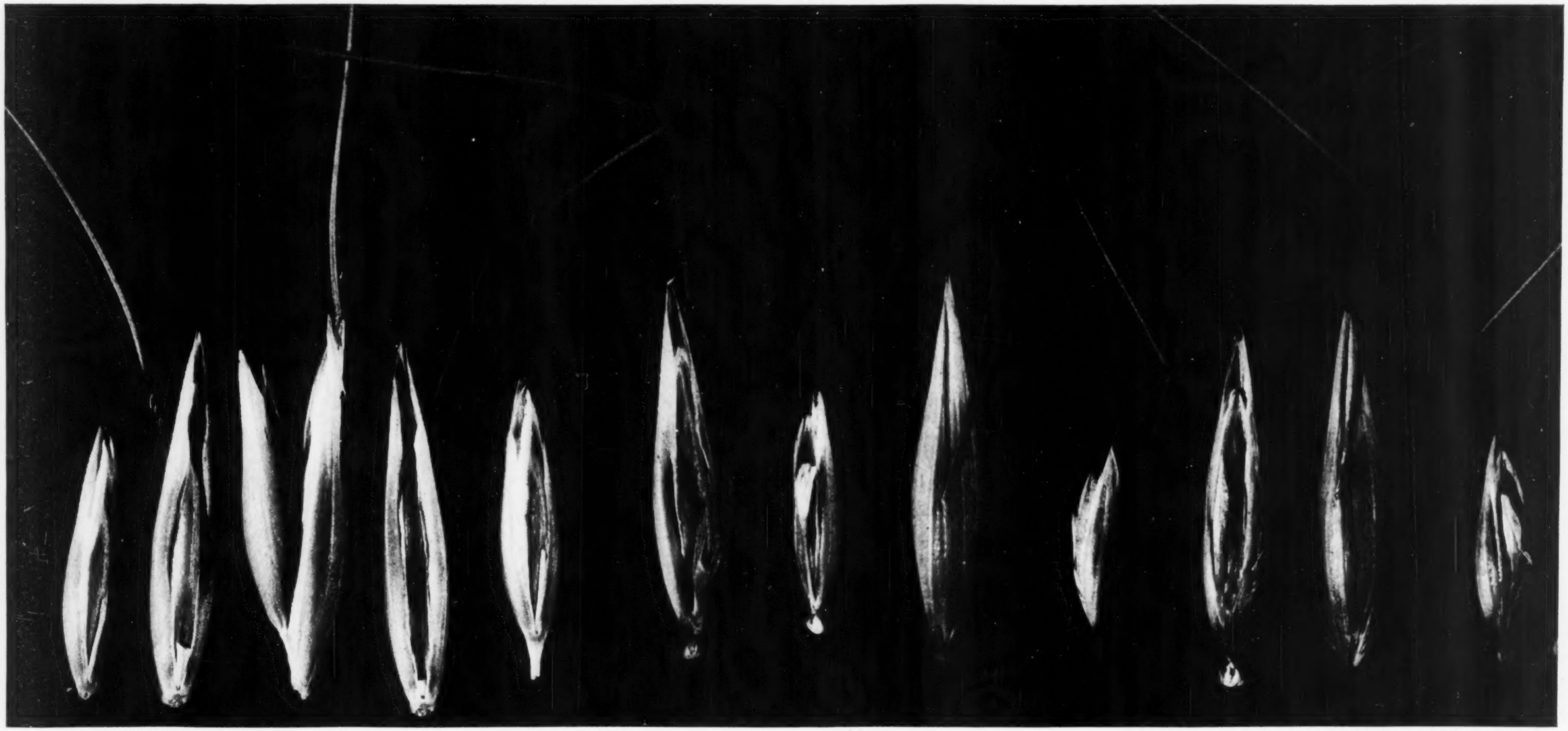
Seed of 10 of the 31 progeny plants grown at Akron in 1923 was sown at the Aberdeen (Idaho) Substation in 1924. Of these 10 plants 9 were classed as homozygous-fatuoid or heterozygous-fatuoid, and 1 was the heterozygous-cultivated plant previously mentioned. Seven of the nine supposedly homozygous-fatuoid parents produced 95 progeny plants, all of the parental form. The progeny of the remaining two, however, showed segregation. One produced progeny, 15 of which were classed as homozygous-fatuoid or heterozygous-fatuoid, and 1 as homozygous-cultivated or heterozygous-cultivated. The other produced 8 progeny plants classed as homozygous-fatuoid or heterozygous-fatuoid, and two classed as homozygous-cultivated or heterozygous-cultivated. The tenth parental individual, classed as homozygous-cultivated or heterozygous-cultivated, produced 18 progeny, all of which resembled the parent, indicating it was probably a homozygous-cultivated.

In addition to the above, data were also obtained on 8 plants grown at Akron (Colo.) Field Station in 1923 from phenotypically homozygous-fatuoid kernels which were separated from the threshed seed of a head selection row of the same original strain, Kansas No. 6076, grown at that station in 1921. Six of these plants were homozygous-fatuoid or heterozygous-fatuoid, and two were classed as homozygous-cultivated or heterozygous-cultivated.

In 1924, seed of each of these 8 plants was sown at the Aberdeen Substation. Four of the 6 parental plants classed as homozygous-fatuoid or heterozygous-fatuoid produced a total of 25 progeny, all of the parental form. The other two of the six produced only a few progeny but these segregated as follows: 4 homozygous-fatuoid or heterozygous-fatuoid, and 3 homozygous-cultivated or heterozygous-cultivated. The seed of the 2 parent plants classed as homozygous-cultivated or heterozygous-cultivated forms produced a total

*Kansas Agricultural Experiment Station accession number.

Florets of the homozygous- or heterozygous-fatuoid forms occuring in the Fulghum and Burt varieties of *Avena byzantina*. In both Fulghum and Burt two types are found which differ in length of hairs on the basal callus and supported rachilla segment. The difference in hair length is very marked and undoubtedly has a genetic basis as the two forms appear to be heritable in both varieties.



Heterozygous Burt | Short Haired Fulghum | Long Haired Fulghum

FATUOIDS IN BURT AND FULGHUM

Figure 4

The intermediate (Heterozygous-cultivated) form occurring in Burt is shown at the left. In many cases the separation of the florets of the spikelet is typical of cultivated derivatives of *Avena sterilis*. The Fulghum strain having black lemmas has short hairs. However, not all short-haired Fulghum is black. The genetic behavior of the fatuoids in Fulghum and Burt, both varieties of *A. byzantina,* indicates their relationship. In both varieties the inheritance of the fatuoid form is more complex than that of similar forms in derivatives of *A. fatua.*

Aurora Navarro Dwarf Culberson

FATUOID FORMS IN THREE VARIETIES OF AVENA SATIVA

Figure 5

Aurora and Culberson are classed as varieties of *A. sativa*. The behavior of the fatuoid forms found in them is typical of all those found in derivatives of this species, being due to a single factor. The fatuoids in Navarro (Ferguson Navarro) behave more like those in Burt and Fulghum and suggest that possibly this variety may have originated in a similar manner.

of 29 progeny which segregated, 23 resembling the parents and 6 being classed as homozygous-fatuoid or heterozygous-fatuoid. A spikelet and florets of the heterozygous-cultivated Burt form is shown in Figure 4.

An interesting observation regarding the length of the hairs on the callus or base of the Burt fatuoid was made in 1924 at the Aberdeen Substation. It was noted that some produced long hairs on the callus and others very short hairs. The two types were grown in the greenhouse at the Arlington Experiment Farm during the winter of 1924-1925 and apparently have bred true for the long and short hairs, respectively. A similar variation in length of hairs also was observed in Fulghum fatuoids. The two types are shown in Figure 4.

The limited data obtained for the Burt fatuoids are sufficient to prove almost conclusively that these forms genetically are similar to those occurring in the Fulghum variety. This conclusion adds weight to the theory previously advanced by Coffman, Parker, and Quisenberry[3] that the Burt and Fulghum varieties are genetically similar, and that probably their origin is similar if they have not actually had a common ancestry. Both Fulghum and Burt are characterized by marked variability in plant and kernel characters, this apparently being true of most strains of *Avena byzantina*. This variability may also explain the genetic instability observed in fatuoid forms occurring in these varieties, the fatuoid forms varying to some extent in a manner similar to the parent varieties.

Etheridge[11] and others have classified Fulghum and Burt as belonging to the red oat group, *Avena sterilis* L. Coffman, Parker and Quisenberry[3] have shown that Europeans long have designated the cultivated forms of red oats by a definite specific name, just as the derivatives of *A. fatua* L. have been designated by the specific name *A. sativa* L. Of several names used by Europeans for cultivated forms of *A. sterilis* L., these authors have suggested *Avena byzantina* C. Koch, as the most appropriate on the basis of priority.

Navarro (Ferguson Navarro)

The three fatuoids found in Navarro, whose histories already have been stated, were like practically all other false wild oat forms in resembling the parent variety. Progenies from these plants were grown at the Aberdeen Substation in 1924. The two plants selected at the Arlington Experiment Farm were homozygous in their breeding behavior but, of the 18 progeny plants produced from the plant found at the Akron Field Station, one apparently was a heterozygous-cultivated form, proving that the parent plant was heterozygous-fatuoid, rather thar homozygous-fatuoid. Little is known concerning the origin of the Navarro variety. Botanically, it usually has been considered as belonging to *Avena sativa*. Practically nothing is known of its genetic constitution, but the breeding behavior of the few fatuoid forms found in the variety may indicate that its origin is similar to that of Fulghum and Burt. However, further experiments will be necessary to determine this point.* Spikelets and florets of the Navarro fatuoid are shown in Figure 5.

Aurora

At the Arlington Experiment Farm in 1921, 12 fatuoid plants appeared among the 24 individuals in a plant row of the Aurora variety. Progenies were grown from each of these plants at the Aberdeen Substation in 1922. The breeding behavior of the 12 fatuoid individuals from the original plant row indicated that they were genotypically homozygous, as all the 185 progeny plants were of the parental form. The breeding behavior of the other 12 plants of the original plant row was very similar to that previously reported by Garber[14] from his studies of these forms in Aurora. Of

*Additional information obtained since this paper went to press indicates that the Navarro fatuoid is similar in genetic behavior to those occurring in the Burt and Fulghum varieties.

these 12 plants, 5 produced 112 progeny, all of which appeared to be of the parental form. The remaining 7 plants produced a total of 160 segregating progeny, 48 of which were classed as homozygous-fatuoid and 112 as homozygous-cultivated or heterozygous-cultivated.

Progenies were grown from all plants of one segregating progeny row of the 1922 crop of Aurora. In 1922, 8 of the plants in this particular row were classed as homozygous-fatuoid, and 117 as homozygous-cultivated or heterozygous-cultivated. A total of 196 progeny plants was produced from the 8 homozygous fatuoids, all of which were of the parental form. Table IV presents the data obtained on the breeding behavior of the other 17 plants. Of these, 3 produced a total of 73 progeny plants, all of which were classed as belonging to the homozygous-cultivated group. The remaining 14 parental plants produced 73 homozygous fatuoids and 256 plants classed as homozygous-cultivated or heterozygous-cultivated. A spikelet and floret of the Aurora fatuoid are shown in Figure 5.

Note also was made of the extent of awning in the 17 parental plants classed as homozygous-cultivated or heterozygous-cultivated, breeding data for which are given above. The awning data for both parents and progeny are shown in Table IV. From these data it is evident that in some heterozygous-cultivated forms awns are not always borne on the lower florets of all spikelets. This indicates that in Aurora, except for homozygous-fatuoid individuals, little or no correlation exists between the nature and number of awns in the parent and its genetic stability or instability as indicated by the breeding behavior of its progeny. This is contrary to observations as interpreted by many previous investigators of the subject. Usually it has been observed that the heterozygous-cultivated form, the heterozygous false wild form of Garber[11], bears twisted and geniculate awns, at least on the lower floret of the spikelet.

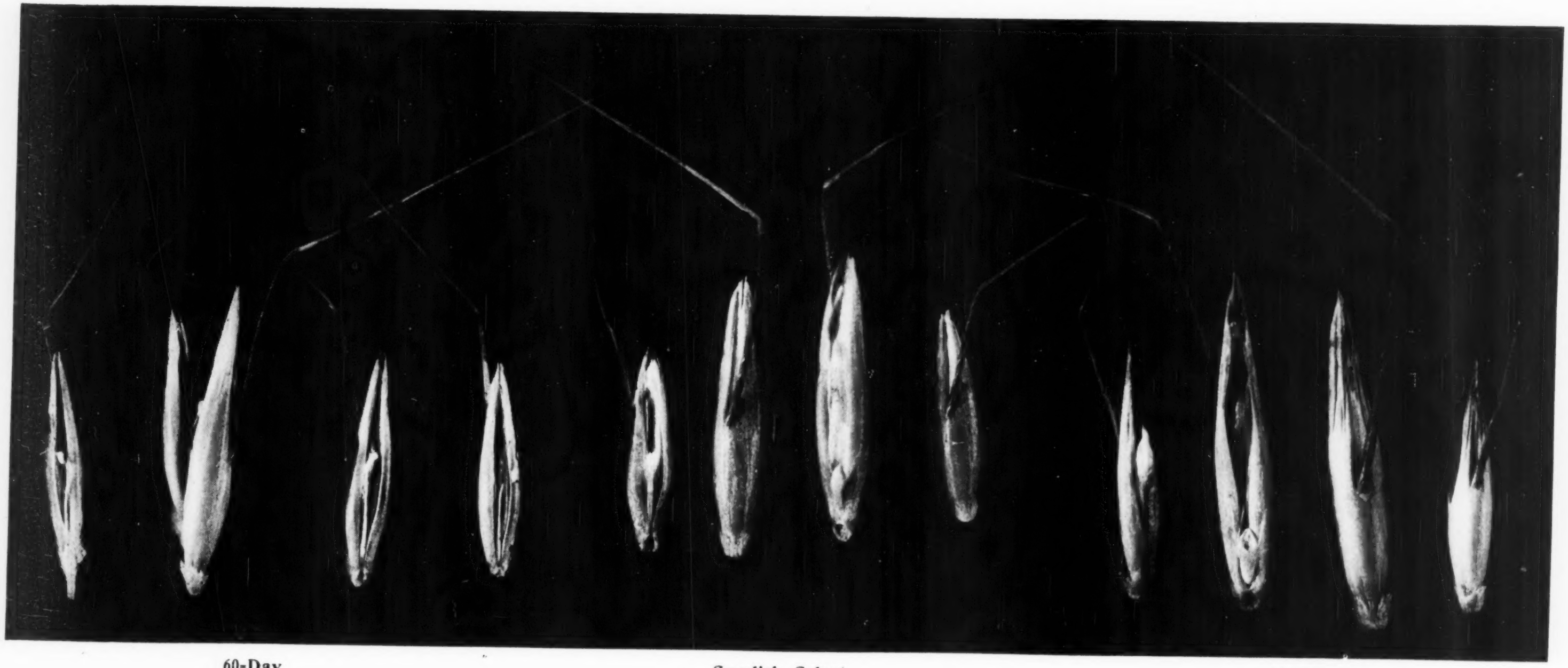
Gante[13], who studied the nature and abundance of awns in the heterozygotes of certain fatuoid forms, observed that in a variety normally bearing some awns the fatuoid heterozygotes of the variety bore more numerous awns than did the cultivated plants. He also observed that in varieties with slight tendency to awning the awns often are first observed among the heterozygous-cultivated aberrants. Apparently he believed that in a variety normally bearing partly 2-awned spikelets the character of awning was independent of and not necessarily linked with the "mutating character" resulting in the appearance of the fatuoid.

Garber[14] stated his inability to differentiate satisfactorily between the cultivated and the heterozygous-cultivated (heterozygous-false wild) forms in the Aurora variety. The observations of Garber and of the writers are to some extent at least explainable on the basis of Gante's results. Aurora is almost a completely awnless variety, for which reason the heterozygous-cultivated individuals of Aurora can not be distinguished always from the normal or cultivated ones on the basis of awns.

Culberson

In the summer of 1921, two typical fatuoid plants were found in plats of the Culberson variety at the Arlington Experiment Farm. A total of 34 progeny, all of which were homozygous fatuoids, was grown from these plants at the Aberdeen Substation in 1922. No homozygous-cultivated or heterozygous-cultivated forms appeared among these progeny, all being the counterpart of the parental form.

An additional fatuoid plant was found in a plat of Dwarf Culberson at the Arlington Experiment Farm in 1922. A total of 23 progeny plants was grown from this aberrant at the Aberdeen Substation in 1923. All of these progeny also bred true to the parental form. Spikelets and kernels

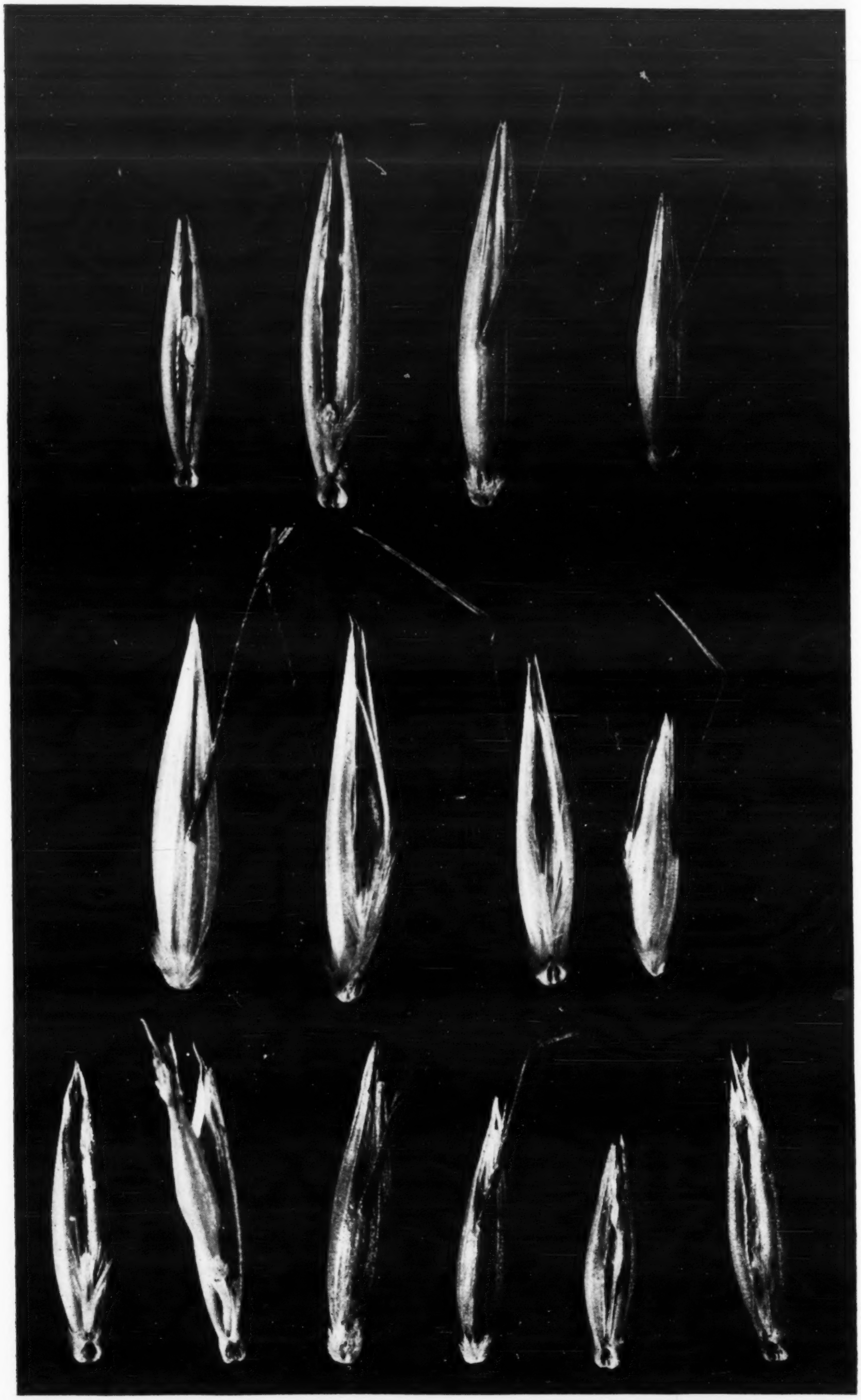


60-Day Swedish Select Victory

HOMOZYGOUS FATUOIDS AND AN UNUSUAL INTERMEDIATE FORM

Figure 6

The "homozygous fatuoids" in Victory and Swedish-Select breed true. The form found in Sixty Day differs in the manner of floret separation from all other fatuoids described in this paper. There is some doubt as to the justification for calling this form a fatuoid as it appears to be intermediate between *Avena fatua* and *A. sterilis*. After ripening it persists much longer than the other fatuoids described and usually on shattering the entire spikelet falls at the same time. Usually in all the other forms described, the second floret of the spikelet separates before the primary floret.



Ruakura (Center) Hull-less (Below)

FATUOIDS IN THREE VARIETIES

Figure 7

The fatuoid found in Cornellian has not been studied genetically but it seems likely that it will breed true, as has been found the case in similar forms in the other *Avena sativa* varieties. Ruakura is usually classified as belonging to *A. sativa* but it was developed from a "natural variation" in Red Algerian, a variety of *Avena byzantina*. The "homozygous fatuoid" form in Ruakura has so far bred true, the kernel characters in this form resemble those of Fulghum. The fatuoid in Hull-less (*Avena nuda*), was found in the progeny of a single panicle, and no data are available as to its inheritance.

of the Dwarf Culberson fatuoid are shown in Figure 5.

Sixty-Day

In 1922, a false wild panicle was found in an exhibit bundle of a commercial variety said to be Sixty-Day at the Washington County Fair at Akron, Colo. In most characters this individual was typical of the unusual fatuoid forms, but it differed markedly in that its seeds were not dropped immediately on ripening, and a decided tendency existed in many spikelets for the florets to remain together in threshing, as is true with derivatives of *Avena sterilis*. In 1923, 20 progeny plants were grown at the Akron Field Station and all were like the parent, no segregation resulting. One hundred seeds were sown at the Aberdeen Substation in the spring of 1924. All but one of the 78 progeny plants produced from this sowing likewise were like the parent plants. The peculiar characters of the original selection persisted in all the progeny, except the one which appeared to be intermediate. These characters differentiate this aberrant from all other false wild oat forms described in this paper. Spikelets and florets of this very unusual false wild form, which possibly can not be considered as fatuoid due to its distinct characters, are shown in Figure 6.

Swedish Select

In the summer of 1923, a number of fatuoids were found in a plant row of the Swedish Select variety growing at the Aberdeen Substation. These false wilds were similar to those in other varieties and they also resembled their parent variety, Swedish Select, in color, plumpness, etc. Progenies were grown from these fatuoids in 1924. The 5 homozygous fatuoids produced a total of 91 progeny plants, all of which were of the parental form. Of the 14 parental plants classed as heterozygous-cultivated, all proved to be truly heterozygous in breeding behavior. A total of 236 progeny was produced from these 14 parental plants. Of this number 53 were homozygous-fatuoid, 108 were classed as heterozygous-cultivated and 75 as homozygous-cultivated Swedish Select. A segregation ratio of 1:2:1 is indicated. The three parental normals from the original 1923 head row bred true, producing a total of 51 plants. Spikelets and florets of the Swedish Select homozygous-fatuoid oat are shown in Figure 6.

Ruakura

In the spring of 1923 one kernel with fatuoid characters was found in a 12-gram sample of the Ruakura variety received from the Office of Cereal Investigation, Washington, D. C., for sowing in the nursery at the Akron Field Station. The plant grown from this kernel had all the characters typical of the usual fatuoid. Some 20 seeds from this plant were sown at the Aberdeen Substation in 1924. A total of 12 progeny plants was produced, all of which showed the characters of the parental Ruakura fatuoid. It is very similar in kernel characters to those found in the Fulghum variety. Possibly the most marked difference is that in general the kernel of the Ruakura fatuoid is lighter in color than that of the usual Fulghum fatuoid. The Ruakura fatuoid is shown in Figure 7.

Victory

Two fatuoids were selected from the Victory variety in August, 1924. The first was discovered in a strain of Victory at the Aberdeen Substation. A second plant was selected by the senior writer in company with R. D. Lewis, of the Plant Breeding Department of Cornell University, while making an inspection tour in certifying seed oats in Western New York. These plants were typical fatuoids and resembled the Victory parent in many characters. According to Garber[14], who previously has observed Victory fatuoids, these should breed as homozygotes or pure recessives. Spikelets and florets of the Victory fatuoid are shown in Figure 6.

Cornellian

In 1924, members of the staff of the Plant Breeding Department of Cornell University found a number of fatuoids in the Cornellian variety, both on the Experiment Station grounds at Ithaca, and in fields of Cornellian in New York State. The lemmas of the Cornellian fatuoids were gray in color and slender in form, thus resembling those of the parent variety. Most of the fatuoids from varieties of *Avena sativa* have been found to breed as pure recessives. As the Cornellian is classed as belonging to the *A. sativa* group, it is believed that these fatuoids probably will breed true. Spikelets and florets of the Cornellian fatuoid are shown in Figure 7.*

Naked (Hull-less)

The writers are indebted to Matthew Fowlds, Seed Analyst of the South Dakota State College of Agriculture and Mechanic Arts, for the following statement† regarding his observation on the finding of a fatuoid while examining threshed seed of the naked or hull-less oat variety, C.I. No. 771, at Brookings in 1924:

> The variety of hull-less oats in which the false wild form occurred can be distinguished from other hull-less varieties by the frequent occurrence of two awns per spikelet. In 1923, the variety was represented in our nursery by a single head row. The fatuoid form therefore occurred in a row which was removed only one generation from a single head selection. The hull-less variety has never been grown near wild oats at Brookings, but it has occasionally been grown near hulled varieties. Unfortunately, the row in which it occurred was not carefully examined before threshing and the aberrant form was first observed in the threshed seed. About 150 seeds were found so it would seem reasonable to conclude that the form was represented by a single plant. The seed is somewhat variable, and perhaps is heterozygous. Several of the seeds appear to be incompletely hulled and one has a long rachilla. These characters would indicate its origin from the hull-less variety in which it was found.

Florets of the fatuoid observed by Mr. Fowlds are shown in Figure 7.

Discussion

From the review of literature and the data presented it appears that the occurrence of the aberrant fatuoids in cultivated oat varieties is common. The present paper is an account of such forms occurring in three distinct oat groups and in 11 varieties. These are *Avena sativa* L., varieties Aurora, Culberson, Sixty-Day, Swedish Select, Victory, Navarro (Ferguson Navarro), Ruakura and Cornellian; *A. byzantina* C. Koch, varieties Fulghum and Burt; and *A. nuda* L., variety Naked (Hull-less). These aberrants also have been observed by the writers in the progeny of crosses and in the peculiar multiflorous variation of Burt described by Coffman and Quisenberry[2]. Criddle[9] has shown that they occur in Storm King, a variety of the subgroup, *A. sativa orientalis*. It therefore seems likely that fatuoids occur in all species of cultivated oats and in representative varieties of practically every group now grown in the United States.

At one time it was believed that there was some danger of these fatuoids becoming a noxious weed similar to *Avena fatua*, but at the present time the results of Fischer[12], Criddle[9], and Garber and Quisenberry[15] are accepted as indicating that there is little reason for fearing that such forms may escape and become a pest. As previously noted, the immediate germination of the false wild makes its control relatively simple. On the other hand, the situation does have some elements of danger, and in some cases may prove serious. In May, 1923, the senior writer made a field trip through the Southeastern States and into Texas. In this area fatuoid forms were observed in nearly every commercial field and experimental plat of Fulghum examined. In the vicinity of Substation No. 6, located near Denton,

*The spikelets for photographing were furnished by courtesy of the Plant Breeding Department of Cornell University.

†In letter to T. R. Stanton dated October 29, 1924.

Tex., they were particularly numerous. In that locality it is customary to grow oats from volunteer seeding. This easily could account for the great number of false wilds occurring there in commercial fields. The aberrants ripen early and drop practically all of their seeds, which then are ready to germinate and grow whenever conditions become favorable. This enables these forms to become progressively more numerous from year to year. Many of the false wild plants also were observed along the borders of plats in which the Fulghum variety had been grown the previous year. Along the banks of a large, open drainage ditch passing through the farm on the Substation at Denton, false wild oats appeared to be prevalent. If undisturbed over a period of years and allowed to volunteer and scatter their seed, it seems possible that false wild or any other oat form might become abundant under favorable conditions.

Whether these false wild types eventually will become a serious weed pest under conditions above described remains to be seen. From one year's rather limited observations in that section, the senior writer was led to suspect that they might become a pest. However, as noted, the absence of delayed germination in the fatuoid renders control far less difficult than is true of the common wild oat, *Avena fatua*, and it is believed that by the use of a rational system of crop rotation practically all danger of their becoming very prevalent is precluded.

Some writers have stated that difficulty was experienced in separating the heterozygous-cultivated from the homozygous-cultivated forms. Garber[14] has shown that such forms in the Aurora variety are difficult to differentiate phenotypically. Heretofore, apparently all have found that recognition of homozygous fatuoids was not difficult, as they could be classified on the basis of their phenotypic characters. Also, little difficulty has been found in the separation of the genetically homozygous-fatuoid from the heterozygous-fatuoid forms of other writers according to their phenotypes. The kernel characters of the former have been described as being similar to those of the kernels of *Avena fatua*, and the kernel characters of the latter usually have been shown to be similar to those of the cultivated oat. The heterozygous-cultivated individuals usually have been shown to bear a twisted and geniculate awn and a few basal hairs, especially on the lemma of the lower floret of the spikelet. The upper florets of the spikelet of the genetically heterozygous forms usually are characterized as being similar to the upper florets of the cultivated oat.

The data in this paper cover a period of six years and are based on more than 5,000 plants. They indicate that in many cases the above procedure for the separation of genetically homozygous-fatuoid and heterozygous-fatuoid individuals by phenotypic characters can be followed with accuracy. In other cases this method of differentiating the heterozygous from the homozygous-fatuoid aberrants is utterly untrustworthy. The data show that some individuals which, on the basis of their phenotypic characters, were considered as homozygous fatuoids, did not breed true. In some varieties it is as yet impossible to recognize different genotypic fatuoid individuals on the basis of their phenotypic characters. The results of most investigators apparently differ in this respect from those obtained by the writers with fatuoids occurring in Fulghum and Burt. In the type previously recognized it was considered that a single factor difference existed between the cultivated and the homozygous-fatuoid forms. The F_2 generation of a cross including such a form as one parent has been shown by Garber[14] and other investigators to segregate in the usual manner of a simple monohybrid or into the ratio of 1 homozygous-cultivated, 2 heterozygous-cultivated, and 1 homozygous-fatuoid plant. Previous to this time apparently no reports of a variance from

this rule have been published. It is interesting in this connection, therefore, to learn that heretofore all genetic investigations into the breeding behavior of fatuoid or false wild forms apparently have been with forms occuring in varieties belonging to the oat group, *Avena sativa*, or the subgroup, *A. sativa orientalis*. This fact, together with the data presented on the breeding behavior of such forms selected from the Aurora, Swedish Select and similar varieties which belong to the *A. sativa* group, has led the writers to suspect that most, if not all, such forms from varieties belonging to *A. sativa* and *A. sativa orientalis*, may be of a similar genetic constitution.

The results obtained in the study of the fatuoid forms found in the varieties of *Avena byzantina* differ from those obtained in studies conducted with forms from varieties of *A. sativa*. The results obtained are sufficiently conclusive to indicate that at least some of the aberrants occurring in Fulghum differ from the cultivated form by more than one factor. As no hybridization experiments have been conducted the exact number of factors has not been determined. The evidence, however, gives some reason for the belief that more than a two-factor difference exists between some of the fatuoids and the cultivated forms of Fulghum, for when segregation resulted among the progeny of heterozygous-fatuoid plants, in many cases a ratio approaching 15 or more individuals of the parent fatuoid type to 1 of the cultivated was produced. In subsequent generations many of these so-called cultivated individuals, however, again produced segregating progeny, which fact might indicate an even greater difference than that of the 15 to 1 ratio.

Fatuoid forms probably are more numerous in Fulghum and Burt, particularly in Fulghum, than in most of the other cultivated varieties of oats. This is interesting in view of the fact that these varieties generally are considered as derivatives of the wild red oat, *A. sterilis*, rather than of *A. fatua*, which the fatuoids most resemble.

In Fulghum fatuoids, floret separation usually takes place by disarticulation at the apex of the rachilla segment connecting the 2 florets of the spikelet. In *Avena sterilis* and its supposed derivatives separation of the florets of the spikelets usually results by basifracture, that is, fracture at the base of the connecting rachilla segment. The fatuoid forms in Burt apparently are similar to those occurring in Fulghum. If it be accepted that these varieties are derivatives of *A. sterilis*, then "wild like" aberrant forms occurring in them should reasonably be expected to more closely resemble their supposed ancestral wild form. The fact that the "wild like" forms so far observed by the writers more closely resemble *A. fatua* than *A. sterilis*, gives some reason for questioning the belief that all *A. byzantina* varieties are derived from *A. sterilis*, or it raises the question: Are *A. fatua* and *A. sterilis* more closely related with regard to their origin than has been previously considered? The chromosome numbers are reported to be the same for both species, and it is of interest that a "wild like" form somewhat resembling *A. sterilis* occurred in the Sixty-Day, a variety of *A. sativa*. It must be admitted, however, that the data herein presented give reason for believing that the fatuoid forms occurring in the *A. byzantina* varieties, Fulghum and Burt, differ widely genetically from similar forms appearing in the varieties of *A. sativa* which have been investigated so far. Further study of the genetic behavior and cytological study of the chromosomes of fatuoids in general, and especially in those found in Fulghum and Burt, seem desirable. It might be found by cytological investigations that chromosome aberrations account for the appearance of fatuoids in general and possibly for the peculiar breeding behavior of the forms in these varieties.

A few, notably Zade[31] (pp. 217-225) and Tschermak[28], apparently still be-

lieve natural crossing between the wild and cultivated oat the more logical explanation for the origin of these forms, although fatuoids have been observed in districts where *Avena fatua* rarely is found. Natural crossing is known to occur in oats, as is shown by Stanton and Coffman[27]. If cultivated forms exist in which complementary factors are present which when brought together will produce aberrant forms, natural crossing possibly might explain the general occurrence of fatuoids. While most writers have favored mutation as the more logical explanation, the data presented herein lead the present writers to believe that chromosome irregularity is possibly a better explanation for their occurrence.

Chromosome irregularity, it seems, may be capable of causing a high degree of self sterility in the fatuoids from *A. byzantina*, probably resulting in an unusually high percentage of natural crosses. This may explain the peculiar complex breeding behavior of fatuoid forms and their frequent occurrence, especially in Fulghum and Burt. However, in the absence of data regarding the general reaction of such irregularities and definite cytological evidence, this conclusion is merely suggested or offered as a possible working hypothesis.

Conclusions

1. Fatuoids, or false wild forms, were first studied in Europe apparently about forty years ago. In the United States they have been studied less than ten years.

2. These types have been called "Zwischenformen," "Flughaferformen," "false wild" and "fatuoid." The writers prefer the term "fatuoid" as most suggestive of the character of these oat forms.

3. Fatuoid forms usually are similar in many kernel characters to the variety in which they occur, but all fatuoids possess certain other characters in common.

4. The occurrence of these forms in cultivated varieties is not unusual. Descriptions and illustrations of fatuoids which have been found in three distinct oat species and in eleven varieties are presented.

5. On the basis of breeding behavior the writers recognize four distinct genotypes: (1) homozygous - fatuoid, (2) heterozygous-fatuoid, (3) homozygous-cultivated, and (4) heterozygous-cultivated. They experienced some difficulty in differentiating between the genetically homozygous - fatuoid form and the intermediate and cultivated forms, on the basis of awns and basal hairs.

6. Some authors believe that the fatuoid differs from the wild oat, *Avena fatua*, in the character of delayed germination, which has not been found in any of the fatuoid forms. While fatuoids occur frequently in the Fulghum variety, the observations and experiments of the writers lead them to believe that there is little reason to fear their becoming a weed pest.

7. Apparently, genetic variation exists between the aberrant forms found in varieties of *A. byzantina* and those found in varieties of *A. sativa*. Fatuoids found in all varieties of *A. sativa* apparently differ from the cultivated form by a single factor, while those so far studied in *A. byzantina* may differ by several factors.

8. Writers do not agree as to the origin of these forms. Some believe they appear as the result of natural crossing with *Avena fatua*, the common wild oat, while others believe loss-mutation the more logical explanation. From the data presented herein the writers are led to believe that chromosome irregularity may offer a better explanation for their occurrence.

9. A false wild form has been found in Sixty-Day, a variety of the *Avena sativa* group, which in floret disjunction somewhat resembles *A. sterilis*, the supposed progenitor of varieties of *Avena byzantina*. On the other hand, the fatuoids observed in Fulghum and Burt more closely resemble

A. fatua than *A. sterilis*, although these varieties supposedly are derivatives of the latter.

10. In connection with the origin of oat species, it appears interesting that typically fatua-like forms arise in varieties of *Avena byzantina*, supposedly a derivative of *Avena sterilis*, and that a somewhat sterilis-like form has occurred apparently likewise in a variety of *A. sativa*, supposedly derived from *A. fatua*.

TABLE IV. PHENOTYPIC CLASSIFICATION OF THE PROGENY OF 17 PARENTAL HOMOZYGOUS-CULTIVATED OR HETEROZYGOUS-CULTIVATED PLANTS OF THE AURORA VARIETY ON THE BASIS OF PRESENCE AND ABSENCE OF AWNS WHEN GROWN AT THE ABERDEEN SUBSTATION IN 1923

		Classification of progeny phenotypically			
Row No.	Awns few or abundant on parent plant	Total number of plants per row	Homozygous-fatuoid	Homozygous-cultivated or heterozygous-cultivated	Awns few or abundant in homozygous-cultivated or heterozygous-cultivated forms
602	Abundant*	19	5	14	One plant only had some awns.
605	Do	25	5	20	Few or no awns.
606	Do	23	4	19	Few awns.
607	Do	25	5	20	Do
608	Do	24	7	17	Do
609	Do	24	3	21	Few or no awns.
610	Do	25	5	20	Do
611	Do	25	0	25	Do
612	Do	23	4	19	Do
614	Do	22	5	17	Do
615	Do	23	4	19	Do
601	Few or Absent	25	4	21	Do
603	Do	23	0	23	Do
604	Do	25	6	19	Do
613	Do	25	0	25	Do
616	Do	25	7	18	Do
617	Do	23	11	12	Do

*Lower floret awned only, basal hairs very few or absent.

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The Study of Man

Rassenkunde, *ed. by* W. Scheit. Vol. I, Allgemeine Rassenkunde by Dr. Walter Scheidt, privatdozen f. Anthropologie an der Universitaet Hamburg. Pp. 585 with 21 tables and 144 illus., price $7.50. J. F. Lehmanns Verlag, Munich, 1925.

Departing from the traditional and less fruitful scope of physical anthropology, Dr. Scheidt concerns himself largely with the bases of eugenics. Apart froma historical chapter and another on the conception of race, the book is almost wholly devoted to a discussion of heredity and selection. It contains an immense amount of material in condensed form. Some of the data are necessarily open to criticism, but the work as a whole is so sound and comprehensive that it will be very nearly indispensable to those interested in human genetics.

P. P.

NEWS AND NOTES

Anti-Evolution Laws and Education

(An Open Letter to the Governor of Mississippi from the Chancellor of the State University)

February 26, 1926.

Governor H. L. Whitfield,
Jackson, Mississippi.

MY Dear Governor:

I am writing you as Governor of Mississippi, clothed with discretionary power and charged with heavy responsibility in matters touching legislative action, and, also, as President of the Board of Trustees of the University and Colleges, with weighty duties to perform relating to these higher institutions of learning and to the youth of the State attending them. . . . . .

I have waited long in the hope that it would not be necessary for me to approach you in regard to what is known as the Anti-Evolution Bill now before the Senate. But I, too, have a duty that belongs to my position and I, too, feel the weight of responsibility which finally, after patient waiting, has become an imperious command impelling me to lay before you the very serious situation in which the proposed legislation threatens to involve the University. Nor can I forget that the University is the head of the State's system of public education and that, as such, it is under obligation to lead and to let its light shine.

In case the measure to which I refer becomes a law, quite a number of our faculty will be confronted with a cruel ethical dilemma. Either they will have to evade, disregard, or openly violate the law, or, else, they will have to be guilty of intellectual dishonesty. To anyone at all worthy of his position on our faculty both of these courses are simply unthinkable and intolerable.

Law must be respected and obeyed. Violation of the State Law on the part of any instructor in the University is not to be considered for one moment. Such a course, even if there were no higher and better reason for refraining from it, would tie his hands should he undertake to require obedience to University regulations on the part of students. Law must be enforced impartially and absolutely, else we have an immoral atmosphere deadening to the life of the institution, blighting and blasting the young manhood and womanhood of the State.

On the other hand, if the law be observed in letter and spirit, the men who handle certain subjects here must surrender intellectual honesty. And what will become of the student body and the State which it represents if led by hirelings too servile to stand for what they believe? A University is very different from a high school and, indeed, from some colleges. It must be free to investigate without limit and to teach without undue restrictions. Otherwise it is fearfully handicapped and cannot possibly fulfill its mission. It is not worth what it costs. If manned by men who are not intellectually free, I, for one, would prefer that a child of mine should never enter its doors. It would be a curse rather than a blessing. Where men are not free to think and to learn, there is stagnation and death. Honest effort and mental growth are impossible in a cramped and narrow environment.

So largely is the best thought of today, both religious and scientific, favorable to a view of evolution in no way inconsistent with the Christian religion, that practically all of the up-to-date textbooks in sociology, psychology, biology, embryology, and oth-

er important subjects accept the findings of modern science. The effect of the Anti-evolution law would be to deal a staggering blow at the departments named and to endanger the standing of the University in the educational world. It would discount our degrees and work a hardship on our graduates. It would severely cripple the usefulness of the institution as a whole and would all but kill its splendid medical school. It would mean the loss of many students who would be forced to go elsewhere for that which their own State University should afford. It would result in the resignation of some high-minded men who are too honest to be evasive and who prize freedom too much to submit to intellectual bondage. And where could we look to fill their places? Under the circumstances, what sort of a man would accept an appointment? Either one without sense or else without conscience. Can a great State put its University in such a deplorable predicament? Can a great State get its consent to say to its young men, the only hope for its future, that they may not sincerely and honestly search for truth and be utterly loyal to it? While in no sense free-thinkers, let our young men be free to think for themselves.

Let us hold with dogged determination to that good old protestant doctrine of private judgment. No modern dictator is any more welcome than medieval pope. Dictation to one's conscience is extremely objectionable and a monstrous injustice to which no protestant should submit. Present-day persecution is just as abhorrent as Spanish inquisition. In the words of a staunch defender of the Christian faith, "It will, in my view, be an evil day, and a day of calamity, when men are tempted, even by the vision of a holy object, to abate in any region or in the smallest fraction, the authority of conscience, or to forget that the supreme title and the supreme efficacy of truth lie in its integrity."

I wish to make an earnest plea, in behalf of young men and women who are passing through the critical period of student days. For their sakes, and for the sake of both church and state, I plead for patient and sympathetic dealing, for wise and prudent leadership. These aspiring and inquiring young people are in far less danger of making shipwreck of their faith if we encourage them to face all the facts squarely and unafraid than if, with temporizing policy, we shut doors in their faces which they will learn to open later. Let us not, with a mistaken notion of shielding or protecting them, unwittingly lead them into mental dishonesty, confusion of thought or an attitude toward truth destructive to the very fibre of character. Young men of parts, with reverential spirit, and devoted to the churches in which hey were born and which they would serve with ever-increasing pleasure, should not be driven from the fold simply because they feel forced to accept the facts and some of the theories of modern science.

Governor, you will bear me record that, during the nearly two years of my Chancellorship of the University, I have not once, heretofore, asked you to share my burden. I have felt that you had enough of your own, and I have not been willing to add to your load. I trust that I do not appear officious now. Nor do I wish to offend those members of the Legislature and others of my fellow citizens who honestly differ with me. With some of these I am personally acquainted, and I hold them in high esteem. I hope that no word or deed of mine will ever cause injury to the institution of which I am a servant. I am merely trying to save it that it may serve. It were better that loss should come to me and my colleagues than that the University should suffer. . . . . .

ALFRED HUME,
Chancellor of Mississippi State University.

The Feeble-minded in Kentucky

ACTING upon the authorization of the representative leaders of some twenty or more organizations who met in conference at the University of Kentucky just before the Christmas holiday, Professor John F. Smith, presiding officer of the conference, appointed a committee of leading Kentuckians to lay out a program for assisting the state in handling some of its pressing problems.

The function of the committee was to suggest a plan based on thoroughly scientific and humane grounds for handling the problems arising from the increasing number of feeble-minded and criminally insane in the commonwealth.

The personnel of the committee follows: Professor W. S. Anderson, chairman, University of Kentucky; Mrs. W. H. Shanks, Frankfort; Dr. B. F. Robinson, Berea; Mrs. Robert A. Brawner, Frankfort, member Board of Charities and Correction; Dr. J. E. Wells, Cythiana, member State Board of Health; Dr. Frank J. O'Brien, Louisville, Family Service Organization; Mrs. George T. Settle, Louisville.

After careful study of the work done in this and in other states the committee presents the following recommendations which we reprint from the *Lexington Herald*:

TEXT OF RECOMMEDATIONS

1. We recommend the isolation of the criminal insane.

2. We recommend that the State Institution for Feeble-minded be retained for the institutional care of idiots, of idio-imbeciles and the lowest grade of imbeciles. We recommend that the middle and high grade imbeciles who are capable of some training be segregated according to grades and sex in farm colonies where they can, in a measure, be self-supporting and be no longer a menace to the public.

3. We recommend, since it is the inexorable law of heredity that the offspring of imbeciles are themselves imbeciles, that when an inmate of a penal or charitable institution is too deficient mentally to produce and to rear socially adequate children that said inmate be retained in a colony of feeble-minded during the reproductive period or that the discharge or parole be given on condition that the inmate consent to eugenical sterilization.

4. We recommend, as an ideal for welfare and altruistic agencies, since parenthood carries with it the responsibility and obligation of supporting and educating the child, that charitable and welfare funds, county and state aid should be so guardedly dispensed that the recipients thereof may not use these funds to bring into the world an ever-increasing supply of dependents.

5. We recommend that the age of marriage be raised from 12 years for females and 14 years for males, to 16 years for females and 18 for males.

6. To get this program before the citizens of the state and to get their approval we request altruistic organizations of the state to aid in the education and the propaganda necessary to have the recommendations adopted and put into practice.

Berea (Ky.) Citizen.

International Federation of Eugenic Organizations

THE eighth meeting of the International Federation of Eugenic Organizations (formerly the International Commission of Eugenics) was held in Paris on Friday and Saturday, July 2 and 3, 1926, by invitation of the Société Francaise d'Eugénique.

In accordance with the policy adopted in 1925, the future meetings will consist of conferences with set programs, in addition to the usual business meetings of the Commission or Federation.

To Improve Manila Hemp

EXTENSIVE research plans are now under way looking to the production of abaca, or Manila hemp, in the Philippines. It is proposed to establish "centrals" in districts where abaca can be grown. Roads and trails will be built to these centrals, and they will be equipped with large-capacity stripping machines so that thousands of laborers can be freed for other work in the preparation of the fiber. A report received by the United States Department of Agriculture describing the plan says that the work is in the hands of the Philippine Government and a board composed of men representative of all phases of the abaca industry and the Cordage Institute of the United States. The last-named body is an association of leading American cordage factories.

Among the chief drawbacks of the industry at present are its dependence on unreliable labor, the production of fiber from immature and weak-fibered plants, and faulty drying and care of the freshly stripped fiber. To remedy these conditions 4 or 5 grades will be established in place of the present 30 grades, and an improved inspection service will be introduced. Valuable by-products heretofore wasted, such as waste fiber, plant pulp and juices, will be utilized. Efforts to combine rubber growing with abaca have been successful, and it is believed the new system of centrals will serve also as concentration points for rubber products.

—U. S. Dept. Agr. Clip Sheet.

Forty-six Breeds of Dogs Important in United States

THE dog population of the United States is approximately 7,000,000, according to estimates, says the United States Department of Agriculture. The department, however, does not attempt to estimate the value of this livestock, for it is composed alike of animals with mixed or mongrel ancestry and canine aristocrats whose known parentage traces back through generations of purebred stock.

About 60 breeds of dogs are considered of enough consequence to be given a classification in the dog world. Only 46 of this number, however, are of sufficient importance in this country to warrant inclusion in Farmers' Bulletin 1491-F, Breeds of Dogs, just issued by the Bureau of Animal Industry.

A short discussion of each breed gives the reader concise information concerning the history, general characteristics, utility, and distribution of the more important breeds of dogs in this country. A list of breed associations, with the addresses of their secretaries, is also included. A copy of the bulletin may be obtained, as long as the supply lasts, by writing to the United States Department of Agriculture, Washington, D. C.

A Congress of Sexual Science

The Internationale Gesellschaft für Sexualforschung, whose activities have been in abeyance since 1913, has been reorganized, and will hold an international congress in Berlin at the end of September or beginning of October of this year. All the principal nations of the world, and many of the smaller ones, are represented on the Committee on Arrangements. Among other phases of the problem that will be taken up at this congress are the broader ones of anthropology, ethnology, sociology, and eugenics, as well as the narrower range of medical and biological problems that have to deal with sex. Correspondence may be addressed to Geh. San. Rat Dr. Albert Moll, Kurfürstendamm 45, Berlin W 15.